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# LUDWIG AND MODERN PHYSIOLOGY.[1]

## I. INTRODUCTION.

THE death of any discoverer—of any one who has added largely to the sum of human knowledge—affords a reason for inquiring what his work was and how he accomplished it. This inquiry has interest even when the work has been completed in a few years and has been limited to a single line of investigation—much more when the life has been associated with the origin and development of a new science and has extended over half a century.

The Science of Physiology as we know it came into existence fifty years ago with the beginning of the active life of Ludwig, in the same sense that the other great branch of Biology, the Science of Living Beings (Ontology), as we now know it, came into existence with the appearance of the "Origin of Species". In the order of time Physiology had the advantage, for the new Physiology was accepted some ten years before the Darwinian epoch. Notwithstanding, the content of the science is relatively so unfamiliar, that before entering on the discussion of the life and work of the man who, as I shall endeavour to show, had a larger share in founding it than any of his contemporaries, it is necessary to define its limits and its relations to other branches of knowledge.

[1] Founded upon a lecture delivered at the Royal Institution, Jan. 24, 1896.

The word Physiology has in modern times changed its meaning. It once comprehended the whole knowledge of Nature. Now it is the name for one of the two Divisions of the Science of Life. In the progress of investigation the study of that Science has inevitably divided itself into two: *Ontology*, the Science of Living Beings; *Physiology*, the Science of Living Processes, and thus, inasmuch as Life consist in processes, of Life itself. Both strive to understand the complicated relations and endless varieties which present themselves in living Nature, but by different methods. Both refer to general principles, but they are of a different nature.

To the *Ontologist*, the student of Living Beings, Plants or Animals, the great fact of Evolution, namely, that from the simplest beginning our own organism, no less than that of every animal and plant with its infinite complication of parts and powers, unfolds the plan of its existence—taken with the observation that that small beginning was, in all excepting the lowest forms, itself derived from two parents, equally from each—is the basis from which his study and knowledge of the world of living beings takes its departure. For on these two facts—Evolution and Descent—the explorer of the forms, distribution and habits of animals and plants has, since the Darwinian epoch, relied with an ever-increasing certainty, and has found in them the explanation of every phenomenon, the solution of every problem relating to the subject of his inquiry. Nor could he wish for a more secure basis. Whatever doubts or misgivings exist in the minds of "non-biologists" in relation to it, may be attributed partly to the association with the doctrine of Evolution of questions which the true naturalist regards as transcendental; partly to the perversion or weakening of meaning which the term has suffered in consequence of its introduction into the language of common life, and particularly to the habit of applying it to any kind of progress or improvement, anything which from small beginnings *gradually* increases. But, provided that we limit the term to its original sense—the Evolution of a living being from its germ by a *continuous*, not a gradual process—there is no

conception which is more free from doubt either as to its meaning or reality. It is inseparable from that of Life itself, which is but the *unfolding* of a predestined harmony, of a prearranged consensus and synergy of parts.

The other branch of Biology, that with which Ludwig's name is associated, deals with the same facts in a different way. While Ontology regards animals and plants as individuals and in relation to other individuals, Physiology considers the processes themselves of which life is a complex. This is the most obvious distinction, but it is subordinate to the fundamental one, namely, that while Ontology has for its basis laws which are in force only in its own province, those of Evolution, Descent, and Adaptation, we Physiologists, while accepting these as true, found nothing upon them, using them only for euristic purposes, *i.e.*, as guides to discovery, not for the purpose of explanation. Purposive Adaptation, for example, serves as a clue, by which we are constantly guided in our exploration of the tangled labyrinth of vital processes. But when it becomes our business to explain these processes—to say how they are brought about—we refer them not to biological principles of any kind, but to the Universal Laws of Nature. Hence it happens that with reference to each of these processes, our inquiry is rather how it occurs than why it occurs.

It has been well said that the Natural Sciences are the children of necessity. Just as the other Natural Sciences owed their origin to the necessity of acquiring that control over the forces of Nature without which life would scarcely be worth living, so Physiology arose out of human suffering and the necessity of relieving it. It sprang indeed out of Pathology. It was suffering that led us to know, as regards our own bodies, that we had internal as well as external organs, and probably one of the first generalisations which arose out of this knowledge was, that "if one member suffer all the members suffer with it"—that all work together for the good of the whole. In earlier times the *good* which was thus indicated was associated in men's minds with human welfare exclusively. But it was eventually seen that Nature has no less consideration for

the welfare of those of her products which to us seem hideous or mischievous, than for those which we regard as most useful to man or most deserving of his admiration. It thus became apparent that the good in question could not be human exclusively, but as regards each animal *its own good*—and that in the organised world the existence and life of every species is brought into subordination to one purpose—its own success in the struggle for existence.[1]

From what has preceded it may be readily understood that in Physiology, Adaptation takes a more prominent place than Evolution or Descent. In the prescientific period adaptation was everything. The observation that any structure or arrangement exhibited marks of adaptation to a useful purpose was accepted not merely as a guide in research, but as a full and final explanation. Of an organism or organ which perfectly fulfilled, in its structure and working, the end of its existence, nothing further required to be said or known. Physiologists of the present day recognise as fully as their predecessors that perfection of contrivance which displays itself in all living structures, the more exquisitely the more minutely they are examined. No one, for example, has written more emphatically on this point than did Ludwig. In one of his discourses, after showing how Nature exceeds the highest standard of human attainment—how she fashions as it were out of nothing and without tools, instruments of a perfection which the human artificer cannot reach, though provided with every suitable material—wood, brass, glass, india-rubber—he gives the organ of sight as a signal example, referring among its

[1] I am aware that in thus stating the relation between adaptation and the struggle for existence, I may seem to be reversing the order followed by Mr. Darwin, inasmuch as he regarded the survival of organisms which are fittest for their place in Nature, and of parts which are fittest for their place in the organism, as the agency by which adaptedness is brought about. However this may be expressed it cannot be doubted that fitness is an essential of organisms. Living beings are the only things in Nature which by virtue of evolution and descent are able to adapt themselves to their surroundings. It is therefore only so far as *organism* (with all its attributes) is presupposed, that the dependence of adaptation on survival is intelligible.

other perfections to the rapidity with which the eye can be fixed on numerous objects in succession and the instantaneous and unconscious estimates which we are able to form of the distances of objects, each estimate involving a process of arithmetic which no calculating machine could effect in the time.[1] In another discourse—that given at Leipzig when he entered on his professorship in 1865—he remarks that when in our researches into the finer mechanism of an organ we at last come to understand it, we are humbled by the recognition "that the human inventor is but a blunderer as compared with the unknown Master of the animal creation".[2]

Some readers will perhaps remember how one of the most brilliant of philosophical writers, in a discourse to the British Association delivered a quarter of a century ago, averred on the authority of a great Physiologist that the eye, regarded as an optical instrument, was so inferior a production that if it were the work of a mechanician it would be unsaleable. Without criticising or endeavouring to explain this paradox, I may refer to it as having given the countenance of a distinguished name to a misconception which I know exists in the minds of many persons, to the effect that the scientific Physiologist is more or less blind to the evidence of design in creation. On the contrary, the view taken by Ludwig, as expressed in the words I have quoted, is that of all Physiologists. The disuse of the teleological expressions which were formerly current does not imply that the indications of contrivance are less appreciated, for, on the contrary, we regard them as more characteristic of organism as it presents itself to our observation than any other of its endowments. But, if I may

[1] I summarise here from a very interesting lecture entitled "Leid und Freude in der Naturforschung" published in the *Gartenlaube* (Nos. 22 and 23) in 1870.

[2] The words translated in the above sentence are as follows: "Wenn uns endlich die Palme gereicht wird, wenn wir ein Organ in seinem Zuzammenhang begreifen, so wird unser stolzes Gattungsbewusstsein durch die Erkenntniss niedergedrückt, dass der menschlicher Erfinder ein Stümper gegen den unbekannten Meister der thierischen Schöpfung sei".

be permitted to repeat what has been already said, we use the evidences of adaptation differently. We found no explanation on this or any other biological principle, but refer all the phenomena by which these manifest themselves to the simpler and more certain Physical Laws of the Universe.

Why must we take this position? First, because it is a general rule in investigations of all kinds to explain the more complex by the more simple. The material Universe is manifestly divided into two parts, the living and the non-living. We may, if we like, take the living as our Norma, and say to the Physicists, You must come to us for Laws, you must account for the play of energies in universal nature by referring them to Evolution, Descent, Adaptation. Or we may take these words as true expressions of the mutual relations between the phenomena and processes peculiar to living beings, using for the explanation of the processes themselves the same methods which we should employ if we were engaged in the investigation of analogous processes going on independently of life. Between these two courses there seems to me to be no third alternative, unless we suppose that there are two material Universes, one to which the material of our bodies belongs, the other comprising everything that is not either plant or animal.

The second reason is a practical one. We should have to go back to the time which I have ventured to call pre-scientific, when the world of life and organisation was supposed to be governed exclusively by its own Laws. The work of the past fifty years has been done on the opposite principle, and has brought light and clearness where there was before obscurity and confusion. All this progress we should have to repudiate, but this would not be all. We should have to forego the prospect of future advance. Whereas by holding on our present course, gradually proceeding from the more simple to the more complex, from the physical to the vital, we may confidently look forward to extending our knowledge considerably beyond its present limits.

A no less brilliant writer than the one already referred to, who is also no longer with us, asserted that mind was a

secretion of the brain in the same sense that bile is a secretion of the liver or urine that of the kidney; and many people have imagined this to be the necessary outcome of a too mechanical way of looking at vital phenomena, and that Physiologists, by a habit of adhering strictly to their own method, have failed to see that the organism presents problems to which this method is not applicable, such, *e.g.*, as the origin of the organism itself, or the origin and development in it of the mental faculty. The answer to this suggestion is that these questions are approached by Physiologists only in so far as they are approachable. We are well aware that our business is with the unknown knowable, not with the transcendental. During the last twenty years there has been a considerable forward movement in Physiology in the psychological direction, partly dependent on discoveries as to the localisation of the higher functions of the nervous system, partly on the application of methods of measurement to the concomitant phenomena of psychical processes. And these researches have brought us to the very edge of a region which cannot be explored by our methods—where measurements of time or of space are no longer possible.

In approaching this limit the Physiologist is liable to fall into two mistakes—on the one hand, that of passing into the transcendental without knowing it; on the other, that of assuming that what he does not know is not knowledge. The first of these risks seems to me of little moment; first, because the limits of natural knowledge in the psychological direction have been well defined by the best writers, as, *e.g.*, by du Bois-Reymond in his well-known essay "On the Limits of Natural Knowledge," but chiefly because the investigator who knows what he is about is arrested *in limine* by the impossibility of applying the experimental method to questions beyond its scope. The other mistake is chiefly fallen into by careless thinkers, who, while they object to the employment of intuition even in regions where intuition is the only method by which anything can be learned, attempt to describe and define mental processes in mechanical terms, assigning to these terms meanings which science

does not recognise, and thus slide into a kind of speculation which is as futile as it is unphilosophical.

## II. LUDWIG AS INVESTIGATOR AND TEACHER.

The uneventful history of Ludwig's life—how early he began his investigation of the anatomy and function of the kidneys; how he became just fifty years ago titular Professor at Marburg, in the small University of his native State, Hesse Cassel; how in 1849 he removed to Zürich as actual Professor and thereupon married; how he was six years later promoted to Vienna—has already been admirably related in these pages by Dr. Stirling. In 1865, after twenty years of professorial experience, but still in the prime of life and, as it turned out, with thirty years of activity still before him, he accepted the Chair of Physiology at Leipzig. His invitation to that great University was by far the most important occurrence in his life, for the liberality of the Saxon Government, and particularly the energetic support which he received from the enlightened Minister v. Falkenstein, enabled him to accomplish for Physiology what had never before been attempted on an adequate scale. No sooner had he been appointed than he set himself to create what was essential to the progress of the Science—a great Observatory, arranged not as a Museum, but much more like a physical and chemical Laboratory, provided with all that was needed for the application of exact methods of research to the investigation of the processes of Life. The idea which he had ever in view, and which he carried into effect during the last thirty years of his life with signal success, was to unite his life-work as an investigator with the highest kind of teaching. Even at Marburg and at Zürich he had begun to form a *School;* for already men nearly of his own age had rallied round him. Attracted in the first instance by his early discoveries, they were held by the force of his character, and became permanently associated with him in his work as his loyal friends and followers—in the highest sense his *scholars.* If, therefore, we speak of Ludwig as one of the

greatest *teachers* of Science the world has seen, we have in mind his relation to the men who ranged themselves under his leadership in the building up of the Science of Physiology, without reference to his function as an ordinary academical teacher.

Of this relation we can best judge by the careful perusal of the numerous biographical memoirs which have appeared since his death, more particularly those of Professor His [1] (Leipzig), of Professor Kronecker [2] (Bern), who was for many years his coadjutor in the Institute, of Professor v. Fick [3] (Würzburg), of Professor v. Kries [4] (Freiburg), of Professor Mosso [5] (Turin), of Professor Fano [6] (Florence), of Professor Tigerstedt [7] (Upsala), of Professor Stirling [8] in England. With the exception of Fick, whose relations with Ludwig were of an earlier date, and of his colleague in the Chair of Anatomy, all of these distinguished teachers were at one time workers in the Leipzig Institute. All testify their love and veneration for the master, and each contributes some striking touches to the picture of his character.

All Ludwig's investigations were carried out with his scholars. He possessed a wonderful faculty of setting each man to work at a problem suited to his talent and previous training, and this he carried into effect by associating him with himself in some research which he had either in progress or in view. During the early years of the Leipzig period, all the work done under his direction was published in the well-known volumes of the *Arbeiten*, and

[1] His. "Karl Ludwig und Karl Thiersch." *Akademische Gedächtnissrede*, Leipzig, 1895.

[2] Kronecker. "Carl Friedrich Wilhelm Ludwig." *Berliner Klin. Wochensch.*, 1895, No. 21.

[3] A. Fick. "Karl Ludwig." Nachruf. *Biographische Blätter*, Berlin, vol. i., pt. 3.

[4] v. Kries. "Carl Ludwig." Freiburg, Bd. i., 1895.

[5] Mosso. "Karl Ludwig." *Die Nation*, Berlin, Nos. 38, 39.

[6] Fano. "Per Carlo Ludwig Commemorazione." *Clinica Moderna*, Florence, i., No. 7.

[7] Tigerstedt. "Karl Ludwig." Denkrede. *Biographische Blätter*, Berlin, vol. i., pt. 3.

[8] Stirling. "SCIENCE PROGRESS," vol. iv., No. 21.

subsequently in the *Archiv fur Anat. und Physiologie* of du Bois-Reymond. Each "Arbeit" of the laboratory appeared in print under the name of the scholar who operated with his master in its production, but the scholar's part in the work done varied according to its nature and his ability. Sometimes, as v. Kries says, he sat on the window-sill while Ludwig with the efficient help of his laboratory assistant Salvenmoser, did the whole of the work. In all cases Ludwig not only formulated the problem, but indicated the course to be followed in each step of the investigation, calling the worker, of course, into counsel. In the final working up of the results he always took a principal part, and often wrote the whole paper. But whether he did little or much, he handed over the whole credit of the performance to his coadjutor. This method of publication has no doubt the disadvantage that it leaves it uncertain what part each had taken; but it is to be remembered that this drawback is unavoidable whenever master and scholar work together, and is outweighed by the many advantages which arise from this mode of co-operation. The instances in which any uncertainty can exist in relation to the real authorship of the Leipzig work are exceptional. The well-informed reader does not need to be told that Mosso or Schmidt, Brunton or Gaskell, Stirling or Wooldridge were the authors of their papers in a sense very different from that in which the term could be applied to some others of Ludwig's pupils. On the whole the plan must be judged of by the results. It was by working with his scholars that Ludwig trained them to work afterwards by themselves; and thereby accomplished so much more than other great teachers have done.

I do not think that any of Ludwig's contemporaries could be compared to him in respect of the wide range of his researches. In a science distinguished from others by the variety of its aims, he was equally at home in all branches, and was equally master of all methods, for he recognised that the most profound biological question can only be solved by combining anatomical, physical and and chemical inquiries. It was this consideration which led

him in planning the Leipzig Institute to divide it into three parts, experimental (in the more restricted sense), chemical and histological. Well aware that it was impossible for a man who is otherwise occupied to maintain his familiarity with the technical details of Histology and Physiological Chemistry, he placed these departments under the charge of younger men capable of keeping them up to the rapidly advancing standard of the time, his relations with his coadjutors being such that he had no difficulty in retaining his hold of the threads of the investigation to which these special lines of inquiry were contributory.

It is scarcely necessary to say that as an experimenter Ludwig was unapproachable. The skill with which he carried out difficult and complicated operations, the care with which he worked, his quickness of eye and certainty of hand were qualities which he had in common with great surgeons. In employing animals for experiment he strongly objected to rough and ready methods, comparing them to "firing a pistol into a clock to see how it works". Every experiment ought, he said, to be carefully planned and meditated on beforehand, so as to accomplish its scientific purpose and avoid the infliction of pain. To ensure this he performed all operations himself, only rarely committing the work to a skilled coadjutor.

His skill in anatomical work was equally remarkable. It had been acquired in early days, and appeared throughout his life to have given him very great pleasure, for Mosso tells how, when occupying the room adjoining that in which Ludwig was working as he usually did by himself, he heard the outbursts of glee which accompanied each successful step in some difficult anatomical investigation.

Let us now examine more fully the part which Ludwig played in the revolution of ideas as to the nature of vital processes which, as we have seen, took place in the middle of the present century.

Although, as we shall see afterwards, there were many men who, before Ludwig's time, investigated the phenomena of life from the physical side, it was he and the contemporaries who were associated with him who first clearly

recognised the importance of the principle that vital phenomena *can only be understood by comparison with their physical counterparts*, and foresaw that in this principle the future of Physiology was contained as in a nutshell. Feeling strongly the fruitlessness and unscientific character of the doctrines which were then current, they were eager to discover chemical and physical relations in the processes of life. In Ludwig's intellectual character this eagerness expressed his dominant motive. Notwithstanding that his own researches had in many instances proved that there are important functions and processes in the animal organism which have no physical or chemical analogues, he never swerved either from the principle or from the method founded upon it.

Although Ludwig was strongly influenced by the rapid progress which was being made in scientific discovery at the time that he entered on his career, he derived little from his immediate predecessors in his own science. He is sometimes placed among the pupils of the great Comparative Anatomist and Physiologist, J. Müller. This, however, is a manifest mistake, for Ludwig did not visit Berlin until 1847, when Müller was nearly at the end of his career. At that time he had already published researches of the highest value (those on the Mechanism of the Circulation and on the Physiology of the Kidney), and had set forth the line in which he intended to direct his investigations. The only earlier Physiologist with whose work that of Ludwig can be said to be in real continuity was E. H. Weber, whom he succeeded at Leipzig, and strikingly resembled in his way of working. For Weber, Ludwig expressed his veneration more unreservedly than for any other man, excepting perhaps Helmholtz, regarding his researches as the foundation on which he himself desired to build. Of his colleagues at Marburg he was indebted in the first place to the anatomist, Professor Ludwig Fick, in whose department he began his career as Prosector, and to whom he owed facilities without which he could not have carried out his earlier researches; and in an even higher degree to the great chemist, R. W. Bunsen, from whom he derived that training in the exact

sciences which was to be of such inestimable value to him afterwards.

There is reason, however, to believe that, as so often happens, Ludwig's scientific progress was much more influenced by his contemporaries than by his seniors. In 1847, as we learn on the one hand from du Bois-Reymond, on the other from Ludwig himself, he visited Berlin for the first time. This visit was an important one both for himself and for the future of Science, for he there met three men of his own age, Helmholtz, du Bois-Reymond and Brücke, who were destined to become his life-friends, all of whom lived nearly as long as Ludwig himself, and attained to the highest distinction. They all were full of the same enthusiasm. As Ludwig said when speaking of this visit: "We four imagined that we should constitute Physiology on a chemico-physical foundation, and give it equal scientific rank with Physics, but the task turned out to be much more difficult than we anticipated". These three young men, who were devoted disciples of the great Anatomist, had the advantage over their master in the better insight which their training had given them into the fundamental principles of scientific research. They had already gathered around themselves a so-called "physical" school of Physiology, and welcomed Ludwig on his arrival from Marburg as one who had of his own initiative undertaken in his own University *das Befreiungswerk aus dem Vitalismus.*

The determination to refer all vital phenomena to their physical or chemical counterparts or analogues, which, as I have said, was the dominant motive in Ludwig's character, was combined with another quality of mind which if not equally influential was even more obviously displayed in his mode of thinking and working. His first aim, even before he sought for any explanation of a structure or of a process, was to possess himself, by all means of observation at his disposal, of a complete objective conception of all its relations. He regarded the faculty of vivid sensual realisation (*lebendige sinnliche Anschauung*) as of special value to the investigator of natural phenomena, and did his best to cultivate it in those who worked with him in the

laboratory. In himself, this *objective* tendency (if I may be permitted the use of a word which, if not correct, seems to express what I mean) might be regarded as almost a defect, for it made him indisposed to appreciate any sort of knowledge which deals with the abstract. He had a disinclination to philosophical speculation which almost amounted to aversion, and, perhaps for a similar reason, avoided the use of mathematical methods even in the discussion of scientific questions which admitted of being treated mathematically—contrasting in this respect with his friend du Bois-Reymond, resembling Brücke. But as a teacher the quality was of immense use to him. His power of vivid realisation was the *substratum* of that many-sidedness which made him, irrespectively of his scientific attainments, so attractive a personality.

I am not sure that it can be generally stated that a keen scientific observer is able to appreciate the artistic aspects of Nature. In Ludwig's case, however, there is reason to think that æsthetic faculty was as developed as the power of scientific insight. He was a skilful draughtsman but not a musician; both arts were, however, a source of enjoyment to him. He was a regular frequenter of the *Gewandhaus* concerts, and it was his greatest pleasure to bring together gifted musicians in his house, where he played the part of an intelligent and appreciative listener. Of painting he knew more than of music, and was a connoisseur whose opinion carried weight. It is related that he was so worried by what he considered bad art, that after the redecoration of the *Gewandhaus* concert-room, he was for some time deprived of his accustomed pleasure in listening to music.

Ludwig's social characteristics can only be touched on here in so far as they serve to make intelligible his wonderful influence as a teacher. Many of his pupils at Leipzig have referred to the *schöne Gemeinsamkeit* which characterised the life there. The harmonious relation which, as a rule, subsisted between men of different education and different nationalities, could not have been maintained had not Ludwig possessed side by side with that inflexible earnestness which he showed in all matters of work or

duty a certain youthfulness of disposition which made it possible for men much younger than himself to accept his friendship. This sympathetic geniality was, however, not the only or even the chief reason why Ludwig's pupils were the better for having known him. There were not a few of them who for the first time in their lives came into personal relation with a man who was utterly free from selfish aims and vain ambitions, who was scrupulously conscientious in all that he said and did, who was what he seemed, and seemed what he was, and who had no other aim than the advancement of his science, and in that advancement saw no other end than the increase of human happiness. These qualities displayed themselves in Ludwig's daily active life in the laboratory, where he was to be found whenever work of special interest was going on; but still more when, as happened on Sunday mornings, he was "at home" in the library of the Institute—the corner room in which he ordinarily worked. Many of his "scholars" have put on record their recollections of these occasions, the cordiality of the master's welcome, the wide range and varied interest of his conversation, and the ready appreciation with which he seized on anything that was new or original in the suggestions of those present. Few men live as he did, "*im Gaznen, Guten, Schönen,*" and of those still fewer know how to communicate out of their fulness to others.

## III. THE OLD AND THE NEW VITALISM.

Since the middle of the century the progress of Physiology has been continuous. Each year has had its record, and has brought with it new accessions to knowledge. In one respect the rate of progress was more rapid at first than it is now, for in an unexplored country discovery is relatively easy. In another sense it was slower, for there are now scores of investigators for every one that could be counted in 1840 or 1850. Until recently there has been throughout this period no tendency to revert to the old methods—no new departure—no divergence from the principles which Ludwig did so much to enforce and exemplify.

The wonderful revolution which the appearance of the *Origin of Species* produced in the other branch of Biology, promoted the progress of Physiology, by the new interest which it gave to the study, not only of structure and development, but of all other vital phenomena. It did not, however, in any sensible degree affect our *method* or alter the direction in which Physiologists had been working for two decades. Its most obvious effect was to sever the two subjects from each other. To the Darwinian epoch Comparative Anatomy and Physiology were united, but as the new Ontology grew, it became evident that each had its own problems and its own methods of dealing with them.

The old vitalism of the first half of the century is easily explained. It was generally believed that, on the whole, things went on in the living body as they do outside of it, but when a difficulty arose in so explaining them the Physiologist was ready at once to call in the aid of a "*vital force*". It must not, however, be forgotten that, as I have already indicated, there were great teachers (such, for example, as Sharpey and Allen Thomson in England, Magendie in France, Weber in Germany) who discarded all vitalistic theories, and concerned themselves only with the study of the time- and place-relations of phenomena; men who were before their time in insight, and were only hindered in their application of chemical and physical principles to the interpretation of the processes of life by the circumstance that chemical and physical knowledge was in itself too little advanced. Comparison was impossible, for the standards were not forthcoming.

Vitalism in its original form gave way to the rapid advance of knowledge as to the correlation of the physical sciences which took place in the forties. Of the many writers and thinkers who contributed to that result, J. R. Mayer and Helmholtz did so most directly, for the contribution of the former to the establishment of the Doctrine of the Conservation of Energy had physiological considerations for its point of departure; and Helmholtz, at the time he wrote the *Erhaltung der Kraft*, was still a Physiologist. Consequently when Ludwig's celebrated *Lehrbuch*

came out in 1852, the book which gave the *coup de grâce* to vitalism in the old sense of the word, his method of setting forth the relations of vital phenomena by comparison with their physical or chemical counterparts, and his assertion that it was the task of Physiology to make out their necessary dependence on elementary conditions, although in violent contrast with current doctrine, were in no way surprising to those who were acquainted with the then recent progress of research. Ludwig's teaching was indeed no more than a general application of principles which had already been applied in particular instances.

The proof of the non-existence of a special "vital force" lies in the demonstration of the adequacy of the known sources of energy in the organism to account for the actual day by day expenditure of heat and work—in other words, on the possibility of setting forth an energy balance sheet in which the quantity of food which enters the body in a given period (hour or day) is balanced by an exactly corresponding amount of heat produced or external work done. It is interesting to remember that the work necessary for preparing such a balance sheet (which Mayer had attempted, but, from want of sufficient data, failed in) was begun thirty years ago in the laboratory of the Royal Institution by the Foreign Secretary of the Royal Society. But the determinations made by Dr. Frankland related to one side of the balance sheet, that of income. By his researches in 1866 he gave Physiologists for the first time reliable information as to the heat value (*i.e.*, the amount of heat yielded by the combustion) of different constituents of food. It still remained to apply methods of exact measurement to the expenditure side of the account. Helmholtz had estimated this, as regards man, as best he might, but the technical difficulties of measuring the expenditure of heat of the animal body appeared until lately to be almost insuperable. Now that it has been at last successfully accomplished, we have the experimental proof that in the process of life there is no production or disappearance of energy. It may be said that it was unnecessary to prove what no scientifically sane man doubted. There are, however, reasons why it is

of importance to have objective evidence that food is the sole and adequate source of the energy which we day by day or hour by hour disengage, whether in the form of heat or external work.

In the opening paragraph of this section it was observed that *until recently* there had been no tendency to revive the vitalistic notion of two generations ago. In introducing the words in italics I referred to the existence at the present time in Germany of a sort of reaction, which under the term "Neovitalismus" has attracted some attention—so much indeed that at the *Versammlung Deutscher Naturforscher* at Lübeck last September, it was the subject of one of the general addresses. The author of this address, Prof. Rindfleisch, was, I believe, the inventor of the word; but the origin of the movement is usually traced to a work on Physiological Chemistry which an excellent translation by the late Dr. Wooldridge has made familiar to English students. The author of this work owes it to the language he employs in the introduction on "Mechanism and Vitalism," if his position has been misunderstood, for in that introduction he distinctly ranges himself on the vitalistic side. As, however, his vitalism is of such a kind as not to influence his method of dealing with actual problems, it is only in so far of consequence as it may affect the reader. For my own part I feel grateful to Professor Bange for having produced an interesting and readable book on a dry subject, even though that interest may be partly due to the introduction into the discussion of a question which, as he presents it, is more speculative than scientific.

As regards other physiological writers to whom vitalistic tendencies have been attributed, it is to be observed that none of them have even suggested that the doctrine of a "vital force" in its old sense should be revived. Their contention amounts to little more than this, that in certain recent instances improved methods of research appear to have shown that processes at first regarded as entirely physical or chemical do not conform so precisely as they were expected to do to chemical and physical laws. As these instances are all essentially analogous, reference to one will serve to explain the bearing of the rest.

Those who have any acquaintance with the structure of the animal body will know that there exists in the higher animals, in addition to the system of veins by which the blood is brought back from all parts to the heart, another less considerable system of branched tubes, the lymphatics, by which, if one may so express it, the leakage of the blood-vessels is collected. Now, without inquiring into the *why* of this system, Ludwig and his pupils made and continued for many years elaborate investigations which were for long the chief sources of our knowledge, their general result being that the efficient cause of the movement of the lymph, like that of the blood, was mechanical. At the Berlin Congress in 1890 new observations by Professor Heidenhain of Breslau made it appear that under certain conditions the process of lymph formation does not go on in strict accordance with the physical laws by which leakage through membranes is regulated, the experimental results being of so unequivocal a kind that, even had they not been confirmed, they must have been received without hesitation. How is such a case as this to be met? The "Neovitalists" answer promptly by reminding us that there are cells, *i.e.*, living individuals, placed at the inlets of the system of drainage without which it would not work, that these let in less or more liquid according to circumstances, and that in doing so they act in obedience, not to physical laws, but to vital ones—to internal laws which are special to themselves.

Now, it is perfectly true that living cells, like working bees, are both the architects of the hive and the sources of its activity, but if we ask how honey is made it is no answer to say that the bees make it. We do not require to be told that cells have to do with the making of lymph as with every process in the animal organism, but what we want to know is *how* they work, and to this we shall never get an answer so long as we content ourselves with merely explaining one unknown thing by another. The action of cells must be explained, if at all, by the same method of comparison with physical or chemical analogues that we employ in the investigation of organs.

Since 1890 the problem of lymph formation has been

attacked by a number of able workers, among others here in London, by Dr. Starling of Guy's Hospital, who, by sedulously studying the conditions under which the discrepancies between the actual and the expected have arisen, has succeeded in untying several knots. In reference to the whole subject, it is to be noticed that the process by which difficulties are brought into view is the same as that by which they are eliminated. It is one and the same method throughout, by which step by step, knowledge perfects itself—at one time by discovering errors, at another by correcting them; and if at certain stages in this progress difficulties seem insuperable, we can gain nothing by calling in, even provisionally, the aid of any sort of *Eidolon*, whether "cell," "protoplasm" or internal principle.

It thus appears to be doubtful whether any of the biological writers who have recently professed vitalistic tendencies are in reality vitalists. The only exception that I know is to be found in the writings of a well-known morphologist, Dr. Hans Driesch,[1] who has been led by his researches on what is now called the Mechanics of Evolution to revert to the fundamental conception of vitalism, that the laws which govern vital processes are not physical, but biological—that is, peculiar to the living organism, and limited thereto in their operation. Dr. Driesch's researches as to the modifications which can be produced by mechanical interference in the early stages of the process of ontogenesis have enforced upon him considerations which he evidently regards as new, though they are familiar enough to Physiologists. He recognises that although by the observation of the successive stages in the ontogenetic process, one may arrive at a perfect knowledge of the relation of these stages to each other, this leaves the efficient causes of the development unexplained (*führt nicht zu einem Erkenntniss ihrer bewirkenden Ursachen*)—it does not teach us why one

[1] Driesch. "Entwicklungsmechanische Studien": a series of ten Papers, of which the first six appeared in the *Zeitsch. f. w. Zoologie*, vols. liii. and lv.; the rest in the *Mittheilungen* of the Naples Station.

form springs out of another. This brings him at once face to face with a momentous question. He has to encounter three possibilities — he may either join the camp of the biological agnostics and say with du Bois-Reymond, "*ignoramus et ignorabimus*," *or* be content to work on in the hope that the physical laws that underlie and explain organic Evolution may sooner or later be discovered, *or* he may seek for some hitherto hidden Law of Organism of which the known facts of Ontogenesis are the expression, and which, if accepted as a Law of Nature, would explain everything. Of the three alternatives Driesch prefers the last, which is equivalent to declaring himself an out and out vitalist. He trusts by means of his experimental investigations of the Mechanics of Evolution to arrive at "elementary conceptions" on which by "mathematical deduction"[1] a complete theory of Evolution may be founded.

If this anticipation could be realised, if we could construct with the aid of those new Principia the ontogeny of a single living being, the question whether such a result was or was not inconsistent with the uniformity of Nature, would sink into insignificance as compared with the splendour of such a discovery.

But will such a discovery ever be made? It seems to me even more improbable than that of a physical theory of organic evolution. It is satisfactory to reflect that the opinion we may be led to entertain on this theoretical question need not affect our estimate of the value of Dr. Driesch's fruitful experimental researches.

J. BURDON SANDERSON.

[1] "Elementarvorstellungen . . . die zwar mathematische Deduktion aller Erscheinungen aus sich gestatten möchten." Driesch. "Beiträge zur theoretischen Morphologie." *Biol. Centralblatt*, vol. xii., p. 539, 1892.

# ON RECENT ADVANCES IN VEGETABLE CYTOLOGY.

## PART I.

DURING the last quarter of a century a considerable change has passed over the aspect of biology, especially in this country. It was formerly possible for a man to be, fairly at any rate, well up in the two branches of zoology and botany, but this is no longer possible, regarded from our modern standpoint. Specialisation, inevitable owing to the rapid advances which have been everywhere made, has not only effected a practical divorce between these two sciences, but the same disrupting agency is operating continuously in each of them.

None the less is it true, however, that there are certain features of fundamental importance which are shared alike by animals and plants. This community of structure is most clearly recognised within the limits of the individual cells, and it is perhaps nowhere more impressively demonstrated than in the remarkable similarity which exists between the nuclear division as observed in animals and in plants,—a similarity which may extend to the most minute details.

The cell, using the word in *its widest sense*, is, as Haeckel said long ago, emphatically the unit of life. For though the several parts, such as nucleus and the cell-protoplasm, which together constitute a cell, all possess autonomy to a certain degree, it still remains true that it is only when they operate jointly and in harmony that a successful and "going concern," a living individual, is the result. And since we have strong reasons for believing that animals and plants represent the diverging limbs of a stock traceable at the root to a common source, *viz.*, lowly unicellular organisms, it is obvious that the study of the cell, of its structure and of the functions discharged by its various parts, offers an immensely important, though it may well be a very difficult, field for research.

What, we may ask, is the essential structure of the protoplasm, of the nucleus, and of those marvellous bodies, the chromosomes, which reappear at every nuclear division? What is it that initiates the division of a cell or of its nucleus, and why do some cells go through such complex evolutions whilst others seem to adopt a relatively simple course? What is it that determines that the descendants of one cell shall develop differently from those of another, so as to give rise to this or that tissue system? Or again, how is the unicellular condition of an infusorian compatible with an intricate and often highly differentiated organisation?

These and a host of other questions rise and confront us on the very threshold of our inquiry, and the hints which Nature has dropped for our guidance are at best only obscure ones; thus the position of the biological investigator contrasts unfavourably with that of the chemist or physicist, inasmuch as he is generally debarred, owing to the very conditions of the bodies he is dealing with, from having recourse to *direct* experiment; Nature conducts the experiments and he has to remain content with watching the result, analysing the factors and reconstructing the process as best he can. Nevertheless there is, clearly, no fundamental distinction between the (so-called) observational and experimental sciences.

It is, then, only by patient accumulation and careful comparison of *all* the facts that even a proximate solution of the difficulties before us can ever be reached. Much has been done in collecting the data, and a good deal is known both as to the structure of the cell and the phases through which it passes during its existence. And fortunately one generalisation is gradually emerging with increasing clearness from beneath the ever-growing pile of detail, and it promises to prove a guide of no small value, namely, that in those processes which we have reason to regard as fundamentally important there exists a *surprising degree of similarity* between the structural elements of animals on the one hand and of plants on the other. And these points of similarity are now known to be so numerous

and so close that we are almost warranted in drawing the conclusion that the measure of the resemblance will afford a criterion as to the relative degree of importance to be attached to this or that phenomenon of cell life.

It seems almost certain that this similarity is to be interpreted as the result of the evolution along parallel lines of a particular structural arrangement, or, to put it in another way, as being the outcome of the continuous operation of similar forces upon an essentially similar protoplasmic structure. No doubt all the change manifested in protoplasm is ultimately to be ascribed to the effects of forces upon its own material substance; the special point of interest here lies in the *similarity* of the results. It cannot be due to mere accident that the stages in the development of the spermatozoa of a newt should bear a closer resemblance to the corresponding divisions in the pollen-mother-cell of a lily than they do to the rest of the tissue cells in the body of the same newt.

In the present article it is not my purpose to attempt to summarise the vast amount of detail which has accumulated within recent years; my aim is rather to try to indicate the general directions in which the results seem to be tending, and to point out the kind of evidence on which the current views are based. And although I am here especially dealing with the botanical aspect of the questions involved, it will be clear from what has been already said that it will be impossible, and certainly not desirable, to ignore the investigations which have been prosecuted by the zoologists.

And in order to make clear that which is to follow, it may not be superfluous to recapitulate the general relations of nucleus and cell protoplasm as commonly received at the present time. The essential character of all cells, whether animal or vegetable, and whether they exist as free independent organisms, or whether they form more or less highly differentiated colonies, consists in this, the association of a nucleus with a certain amount of cell protoplasm (commonly called *Cytoplasm*, to distinguish it from the nuclear protoplasm). And this is equally true, so far as we have means of determining the question, in the case of those

organisms in which we as yet have failed to recognise a definite nuclear body, for there are reasons for believing that the nuclear substance is in all cases really present, whether it happens to be collected into a specialised mass or not. And it should be remembered that the number of cells supposed to possess what we may term a distributed or discrete nucleus is becoming smaller as our means of investigations improve. Thus according to Wager (1) even Bacteria possess a true nucleus.

I am perfectly aware that attacks have recently been made on the cell-theory as extended to explain the organisation (Whitman, Sedgwick) of animals, and that nobody would assert the cell to the ultimate unit of *living substance.* But neither of these propositions really affects, or is concerned with, the point of view just now before us. We are not here dealing with the wide questions connected with the architecture of the organism as a whole, nor with the equally difficult one, as to what constitutes the ultimate units of living matter, rather we are content just now to study the interaction of the parts which together are capable of carrying on a continuous living existence, which form a living individual, and these parts consist jointly of the nucleus and its surrounding cytoplasm.[1] The occurrence of cell walls is a matter of no importance from a general standpoint, although when present they may profoundly modify the characters of the organism in which they are formed. Many plants are known in which the protoplasm is only delimited by a cell wall from the surrounding medium, while the oftentimes huge protoplasmic mass suffers no internal partitioning, although it contains a vast number of nuclei distributed through it.

Sachs, with characteristic insight, long ago perceived that the presence or absence of cell walls is a matter of only secondary importance. Their sequence and arrangement at the time of their first appearance can be predicted

[1] The researches of Klebs, Acqua, and others have shown that although protoplasm deprived of a nucleus may sometimes even assimilate food and maintain life for a not inconsiderable period of time, it is incapable of division.

from simple geometrical considerations quite independently of the ultimate form which will be finally assumed as the result of specialised growth. And in applying the word *Non-cellular* to those plants in which partition walls do not occur, he merely gives formal expression to the fact that these anatomical structures are absent, although in other respects the plants in question conform with those usually called multicellular, and they are not at all to be regarded as consisting of a single enlarged cell. In fact he has expressly stated that non-cellular plants are really the equivalent of multicellular organisms in which the formation of internal cell walls does not occur. More recently he has introduced the term *Energid* (2) to express the physiological individuality of those units I have here continued to call cells, and he thereby emphasises the fact of their real existence whether any positive anatomical boundaries can be discerned between them or not.

It must however be clearly understood that in formulating the expression energid, Sachs lays especial stress on the dynamical aspect of the relations existing between the cytoplasm and the nucleus. But it will be admitted by most people that a conception of force apart from the material substance on or through which it acts, and by which its operation becomes perceptible to the senses, belongs to the domain of purely abstract ideas. We require to know far more of the nature and structure of protoplasm before we can usefully divorce our conceptions of force from our experience of matter in attempting to ascertain the nature of those physiological causes of which all external form is but the outward and visible sign. Sachs himself, however, escapes the charge of vagueness, by restricting the application of his expression so as to impose a territorial limit to the sphere of influence mutually existing between each nucleus and the surrounding cytoplasm. For him the word Energid embodies the idea that the whole protoplasmic region is partitioned into smaller provinces each dominated by its own nucleus. And although it may be advantageous for the seprovinces to be delimited from each other by cell walls, permitting thereby a more complete independence to

attach to each one severally, the existence of such well-defined boundaries is by no means an indispensable condition of great complexity of organisation. *Caulerpa* amongst the algæ imitates very closely the differentiated form of some of the higher terrestrial plants, without however possessing their corresponding internal structure. Its protoplasm is bounded by an external wall only, and is not internally partitioned. And yet the characters distinctive of the energids in the leaf-like parts are assuredly different from those of the energids which exist in the creeping stem or rootlike fibres. A transition from the condition of *Caulerpa* to that of the higher plants may be seen in *Cladophora*, in which the filamentous body seems, at first sight, to be made up of chains of cells, each of which stands in a definite relation to the general symmetry of the branched plant; nevertheless, closer examination shows that each "cell" is multi-nucleate, and really represents a federation of energids which so act together as to constitute morphological units as far as the external form of the plant as a whole is concerned.

Sachs' conception of the energid has been assailed by some writers, and he has to some extent perhaps invited criticism by formerly affixing a quasi-morphological, as well as a physiological significance to the term. At first sight it may seem difficult to justify its application in those cases in which streaming movement happens to go on in certain layers of the protoplasm, whilst the layer in which the nuclei are embedded is at rest. It is obvious that if we admit, as we can hardly avoid doing, that the nucleus does really exert a directive action over a localised area, the migratory protoplasm (assuming the movement to affect the protoplasm, and not merely the granular bodies contained in it) must be constantly coming within the range of fresh centres of influence. It may perhaps be compared to the case of a person passing from a region presided over by one government into one under the jurisdiction of another. Such a person would naturally be subjected to changed conditions, without however affecting either his own identity or that of the particular political centres through which he may happen to travel.

Strasburger (3) has attempted to define more clearly the position of the individual energid, by proposing to limit its application to the nucleus together with a special part of the cytoplasm which he calls Kinoplasm and which he regards as the proximate seat of the effective manifestation of the forces at work in the cell. He regards the nomadic streaming protoplasm as being mainly charged with the function of providing nourishment for the nucleus and kinoplasm, and he distinguishes it by the special term of Trophoplasm. Strasburger maintains this same distinction between the active Kinoplasm and the nutritive trophoplasm in those cases in which the limits of the several energids correspond with those of the individual cells; and in this he is logical enough, for we know that living cells are not isolated from each other, but that protoplasmic continuity exists between adjacent cells by means of pores in the intervening walls. How far the distinction between kinoplasm and trophoplasm is either justified by observation or demanded by theory is another matter altogether.

But although the conception of energids is a happy one, as enabling us to distinguish discrete individualities in what may at first sight appear to consist of a common structure, it is not to be inferred that the individuals enjoy independence. The great merit of the idea lies in the fact that it serves to narrow down, and hence to render more clearly comprehensible, many important problems which call for a solution before we can hope to grapple successfully with the more advanced questions relating to those forces of a still higher order which control and apparently direct the development of the organism as a whole, or to put it in another way, which determine the course of development which the particular energids shall follow. Such control is plainly apparent at every stage in the life of an organism. Why does growth take place symmetrically so that the energids, cells, or whatever we may choose to call them, so act in unison as to produce a "body fitly joined together and compacted by that which every joint supplieth, according to the effectual working in the measure of every part"? Without some such assumption how is it

possible to account for the fact that in certain embryos which have been mutilated, the surviving cells are enabled to so modify the course of their normal development as to make good the loss, and thus to form a perfect, if somewhat miniature organism? For had there been no mutilation the cells thus concerned would unquestionably not have developed in the same way, but would have fulfilled the allotted task of merely providing for the genesis of their normal tissue products. Or again, why is it that when a lizard's tail is broken off the general *form* of the entire animal is once more reproduced, even though there are important histological and structural (but probably not functional) differences in the new tail as compared with that of the original one (4)?

When differentiation has so far become manifested in an organism that the limits of the several energids are coterminous with the cell walls, a considerable increase in their degree of independence doubtless ensues, but it is, as already stated, by no means absolute, and the examples just quoted support the statement. Whether organisation is the result of, or the factor which determines, the co-ordinate action of the cells is a question which we may safely leave to the future to decide. But perhaps it may be permissible to compare the cell colony which forms the organism to an isolated society in which the caste system prevails. Each caste or cell group is predestined to discharge certain definite offices in the state or the organism. If some indispensable caste should become exterminated, it is obvious that a differentiation and displacement must occur amongst those which survive, and this differentiation might either be readily complete, or it might only arise as a reluctant concession to necessity, just as a willow twig planted upside down in damp soil will form roots at this, its upper, end; though comparison with a twig planted with its basal end in the ground will show how severe a tax the unusual effort has proved.

It has already been said that an energid, and it might also be added, a typical cell, consists essentially of a nucleus and the protoplasm included within a certain area around

it. But we cannot as yet answer the more obvious and, one might think, almost preliminary question as to what the chief functions which are discharged by these two components really may be. It is certain that the existence of a nucleus is essential to morphological development such as is implied in the production of new cells, and very probably also in the further differentiation of those which have already been formed. Instances of this are seen for example in the growth or alteration of the cell wall. Haberlandt (5) some years ago drew special attention to the fact that when local thickening occurred in a cell wall the nucleus commonly moved to this spot, and the present writer has repeatedly observed it during the formation of the hard coat found on many seeds; here the deposition of substance is usually localised on the inner parts of the cell, and the nucleus takes up a corresponding position as soon as the process begins. Korschelt (6) has observed a similar relation to exist during the chitinisation of the membranes of insect cells, and quite recently Istvanffi (*Ber. Deut. Gesel.*, Dec., 1895) has observed that when the tubular hypha of *Mucor* branches, a nucleus is invariably present at the spot whence the branch is arising. Strasqurger (3*a*) has also drawn attention to the same truth, inasmuch as he states that before the opening of the zoosporangium of *Œdogonium*, the nucleus and kinoplasm aggregate in the vicinity of the spot at which the hole is about to be formed.

But perhaps one of the most striking instances of the directive effect of the nucleus as a whole is to be seen in the result of an experiment of Boveri, who asserts that he impregnated a non-nucleated piece of protoplasm of an echinoderm ovum with the sperm nucleus of another species;[1] development ensued, and the larva resembled the paternal form (7).

In discussing the relations which exist, or are supposed to exist, between the cytoplasm and the nucleus, it is clearly of the first importance to know what are the changes which occur in them, and especially in the nucleus, during the

[1] The animals actually employed were Echinus microtuberculatus (male), and Sphaerechinus granularis (female).

growth, maturity and senescence of the cells. Some extremely interesting results in this direction have recently been published by Zacharias (8). An ordinary resting nucleus consists, as all biologists are aware, of a somewhat dense thread-like framework, often spoken of as linin, which usually exhibits copious anastomosis, sometimes to such a degree that it almost forms a spongy texture. In this framework granules are found embedded which react definitely to stains and to solvents; they constitute the nuclein, a phosphorus-containing substance which at the periods of nuclear division undergoes an enormous increase in bulk. The linin is bathed in a more fluid substance, the paralinin. One or more spherical bodies, the nucleoli, are often present in addition to the foregoing constituents, and the nucleus is delimitated from the cytoplasm by a pellicle or membrane. The nucleolus contains, as was shown by Zacharias many years ago, at least two substances, one of which is of an albuminous nature, and is dissolved out on treatment with gastric juice; after peptic digestion has extracted the albumin, a substance is left which Zacharias calls Plastin. Now observation shows that the relative proportion of these two constituents varies considerably at different periods of the life of the cell, and this is of importance in connection with the intricate series of changes which the nucleus passes through during the process of ordinary division. The conviction has slowly been forced upon us within the last few years that there exists a considerable variety amongst the bodies which have been included in the common term of nucleoli. Auerbach (9) showed in 1890 that some of them absorbed certain red dyes with greater avidity than they did certain blue ones, whilst other nucleoli reacted in the opposite manner. He thus distinguished between erythrophil and cyanophil nucleoli. These results have been extended to plants by the investigations of Rosen (10) and others, but especially by Zacharias, who has applied the test of solvents to them, with the result that the difference between the two classes of nucleoli proves to be a much more real one than had hitherto been supposed. And these observa-

tions are specially interesting when considered from the point of view of the great dissentience of opinion which exists between most botanists and zoologists as to the nature and function of the nucleolus. Strasburger, who admitted the correctness of Rosen's statements, considered that the difference between an erythrophil and a cyanophil nucleus was largely one of nutrition, and he instanced in support of his view the difference between the erythrophil nucleolus in the nucleus of the well-nourished oosphere and the cyanophil nucleus of the much smaller, and therefore presumably worse nourished generative cell of the pollen tube. But Zacharias, in criticising Strasburger's views, considers that there is no evidence to prove that the one nucleolus is in a better position than another as regards its nutrition, and it is still more difficult to accept the suggested explanation in those cases in which both forms of nucleoli are concomitantly present.

Zacharias has shown that whereas the erythrophil nucleoli contain albumin and plastin, the cyanophil kind (the "pseudo-nucleoli" of Rosen and others) contain *nuclein*, a substance quite absent from the other class of nucleoli. Rosen in 1892 stated his conviction that his pseudo-nucleoli in reality consisted of chromatic substance (nuclein) and that they contribute to the formation of those remarkable bodies, the chromosomes, which are evolved by the breaking up of the linin framework after the amount of nuclein has greatly increased in it, previous to the division of the nucleus. Now the nucleolus exhibits striking changes both during the growth, and also during the division of the cell and its nucleus. As regards the behaviour during cell growth, the relation of the nucleolus to the other components of the nucleus is highly suggestive, and seems to support the view of those who hold that its function is largely, at any rate, nutritive.

In the embryonic tissue situated at the growing points of plants, the cells are all much alike, differentiation and specialisation only taking place behind these regions. Consequently it is possible to trace the changes which a cell exhibits during its transition from a primitive state to its adult form, and often, further, through the various stages

of senescence and death. Some cells, indeed, are not really useful to the plant of which they form a part, until they are dead, *i.e.*, till the wall of the cell alone remains, whilst from its cavity the protoplasm has disappeared.

The researches of Zacharias and of Rosen, which have recently been published, were directed especially to the behaviour of nuclei in the apical regions of plants, and their results in the main are confirmatory of each other, though the two observers were interested in rather different aspects of the same problem. The nuclei of all actively dividing cells are markedly cyanophil, and this character is especially noticeable just below the active generative cells. At first sight it may seem remarkable that in a fern root the nucleus of the large apical cell is less cyanophil than are the nuclei of the dividing segment cells which have been cut off from it. But the anomaly is only apparent, for though all the cells in the root owe their origin ultimately to the division of the apical cell, it must not be forgotten that the nuclear divisions in the segments which are cut off from it are far more frequent. The segments divide up into a very large number of cells before they finally form permanent tissue cells, and therefore it is not surprising to find that the nucleus of the apical cell, which is the ancestor of them all, contains less nuclein than the more actively dividing descendants. But there are several other significant observations which go to show that in cells which are in a state capable of further division, this faculty is correlated with the presence of nuclein in their nuclei. Rosen found in the roots of the bean and other flowering plants that after the tissues were beginning to show differentiation, the zone of cells forming the pericycle [1] retained, in their nuclei, the characters of embryonic cells, that is to say, that, whereas the nuclei of the rest were losing their cyanophil character and were becoming erythrophil, the pericyclic nuclei retained their nuclein contents. Now the lateral roots arise in this pericyclic layer, and they do so by the differentiation in it of new growing points. Hence these

[1] A zone of parenchymatous cells sheathing the more central wood and bast parts of the vascular strand.

new rootlets can only be developed from cells which still retain, or can re-awaken, embryonic characteristics. Behind the region in which lateral roots arise, the cells of the pericycle lose their cyanophil nature, and here again the loss is first apparent in those cells from which, even normally, no roots would originate, *viz.*, those situated opposite the phloem. It would be interesting to know whether in the case of those roots in which the lateral rootlets arise right and left of the protoxylem (*e.g.*, Cruciferæ) a corresponding difference obtains.

Again, Zacharias noticed that during the development of the guard-cells of the stomata in a number of leaves a similar difference held good. In a simple case, *e.g.*, many Liliaceæ, the mother-cell of the guard-cells is cut off from a cell which is destined at once to form one of the ordinary and relatively large epidermal cells. In this case, whilst the nucleus of the mother-cell of the stoma retains its nuclein contents, the other one rapidly becomes poorer in this constituent, it grows and develops a large nucleolus. The small mother-cell again divides to form the guard-cells of the stoma, and only then does a nucleolus become at all conspicuous, and the nuclein diminish in quantity. And therewith the further capacity for division ceases.

Besides the connection which is shown to exist between a nucleus which is capable of division, and its richness in nuclein, there are certain other facts of importance which demand notice. The nuclei of cells which are actively dividing are commonly characterised by the possession of smaller nucleoli than are those in which no further divisions will take place, but which are still growing in size. In fact Zacharias states generally that, as regards nuclei of cells emerging from the meristem region, the nucleoli first increase to a maximum, that this is accompanied by an enlargement of the nucleus as a whole, which however only reaches its maximum size after the nucleolus has done so, and that the latter body then diminishes faster than does the nucleus as a whole.

Further, Zacharias found that not only is the nucleolus losing substance in those cells which are specialising to

form tracheids, vessels and sieve tubes, but that the nucleus as a whole is losing, and still more rapidly, those substances which are capable of being removed by peptic digestion from the cell. The facts seem to suggest that it is albumin, or some other proteid, which is disappearing; and it is clear that the loss is due to a change in the nucleus itself, irrespective of the amount of nutrition available in the surrounding plasma, since the change is extremely obvious in the degenerating nuclei of sieve tubes, in spite of the fact that they are surrounded by abundant albuminous substances in the slimy contents of the cells. On the other hand, in those cells which are growing in size, preparatory to further divisions, such as in spore-mother-cells, the *increase* in albuminous substances, both in the nucleus generally, and especially in the nucleolus, is strongly marked. Spore-mother-cells, as a rule, pass through a relatively long period of growth, and hence we might perhaps anticipate (as we find to be the case) that they exaggerate the changes seen in the dividing and growing cells of the apical meristem. But I do not wish to lay too much stress on this, because we know that other, and profound, changes occur during the growth of spore-mother-cells, and it is uncertain to what extent the facts just mentioned may be connected with them.

It may possibly be objected that observations like those of Zacharias are open to adverse criticism on the ground that the chemistry, and *à fortiori* the microchemistry, of the proteids and other substances which occur in cells is as yet in such an unsatisfactory condition. But this objection is really not a legitimate one. We know that certain structures in the cell are differentiated by their selective action on certain dyes, and it is to this fact that their recognition was due in the first instance. But we find the action of certain solvents to yield no less definite results. Given a nucleus in a particular condition (as judged by the structure rendered visible by staining), and it will be found that the degree of solubility of its constituent substances is characteristic for the particular stage in the life history of the cell or of the nucleus which may happen to have been selected.

Hence it seems clear that the two methods ought both to be employed; for whilst the staining exhibits more or less completely the structural arrangement of the substances present, the microchemical method not only indicates some at least of the important differences which exist between the different structures revealed by the action of staining, but it teaches us that certain of these same structures are by no means so homogeneous in their nature as one might be led to suppose relying on the evidence derived from staining alone.

But those who pin their faith on stains sometimes seem to forget that they are after all only employing a sort of microchemical method themselves. For the fact that different histological elements of the cell are distinguishable by stais, implies the existence of a chemical dissimilarity between them. And this becomes the more obvious when, owing to periodically recurring changes in the cell, we assert that this or that structure is growing or diminishing. The investigator who is consciously proceeding on microchemical lines is at least not so open to the charge of mere empiricism as are those who look for salvation to hæmatoxylin or the anilin dyes. He may be wrong in supposing, for example, that the phosphorus within the nucleus only occurs in the nuclein, just as he may be in error in assuming that the substance nuclein itself really represents a chemical substance in the same way that sugar does. But he materially advances our knowledge of the cell when he determines the fact that a body which fluctuates in size as does the nucleolus, is composed of two substances or groups of substances one of which is soluble in gastric juice whilst the other is not; and that further, the relative size is, in the first instance, correlated with the amount of substance which the fermentative action of pepsin can render soluble.

It is readily conceded that the bodies we call nuclein, plastin, and the like, possibly may not, as stated already, represent chemical molecules at all. This does not, however, diminish the interest attaching to the proof that this or that substance is at one time present, while at another

time it can be no longer recognised in its former place. Nor does this observation lose in importance when the differences are shown to closely accompany changes in the general characters of the cells themselves.

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# THE MORPHOLOGY OF THE MOLLUSCA.

THE recent publication of a number of new manuals and monographs dealing with the Mollusca offers a favourable opportunity for a review of our knowledge of this great phylum of the animal kingdom. It is not fifteen years since Professor Lankester's classical article on Mollusca was published in the *Encyclopædia Britannica*, yet the contributions to Molluscan morphology since that date have been not only numerous, but in many cases of prime importance.

The older method of inquiry, that of the comparison of types more or less arbitrarily selected from different groups, has been succeeded by investigations more directly influenced by the idea of evolution. The comparison of types has been replaced by the study of groups. The foundations of the morphological edifice were laid upon the former method; the superstructure and details are the result of the latter. Homologies having been to a large extent determined, we now seek phylogenies. It happens also from time to time that the detailed study of a group with the object of reconstructing the phylogeny of its members leads occasionally to the discovery that homologies based on the simple method of anatomical comparison turn out to be nothing more than analogies—recurrent examples of similar modifications.

One result of these phylogenetic inquiries has been the concentration of particular attention upon forms which are presumably the most primitive in each group; and great advances have thus been made in our knowledge. Kowalewsky and Marion, Pruvot, Wirén, and Thiele have enormously extended our acquaintance with the Aplacophorous Isopleura; primitive Prosobranchs (Docoglossa and Rhipidoglossa) have been thoroughly investigated by Haller and Boutan; Bouvier has thrown new light upon the Opisthobranchia by his researches on *Actæon;* Boas and Pelseneer have revolutionised our ideas of the Pteropoda

by their work upon *Limacina* among the Thecosomata, and upon *Dexiobranchæa* and other types among the Gymnosomata; the morphology of the Pelecypoda has been further elucidated by Pelseneer's observations upon *Nucula* and other primitive forms, and important contributions to our knowledge of the Cephalopoda were made during the past year by Huxley and Pelseneer in the case of *Spirula*, that last survivor of the ancient types of Decapod Dibranchiates. We doubt if any equivalent group of the animal kingdom, except perhaps the Echinoderma, has been the subject of such productive researches as the Mollusca during the period under consideration; and certainly the phylogenetic method of inquiry has attained no greater triumphs than in the hands of Bouvier, Haller, Pelseneer, and other investigators of the Gastropod and Lamellibranch series.

In the present article I propose to deal more especially with recent contributions to our knowledge of the Molluscan nervous system, reserving a fuller consideration of other questions for a later article.

There is one writer, however, whose views must first of all be dealt with, as on a great number of fundamental points they are opposed to all current conceptions of Molluscan morphology. These views merit some detailed consideration, moreover, for they are based on propositions which are not without a certain appearance of plausibility, and may well serve as test-questions by which to examine into the accuracy of the homologies which have been generally admitted to exist between the different sections of the Molluscan phylum.

Thiele has published his views in a series of lengthy papers, the references to which will be found in the bibliography (23, 24, 25). He regards the Mollusca and Annelida as direct descendants of Polyclad Turbellarians, and his identifications of homologous organs in the different Molluscan groups are determined, not by a direct comparison of the organisation of these types one with another, but by independent comparisons of the organisation of the different Molluscan types with that of sucker-bearing Polyclads. The group Mollusca is thus made to lose its

compactness, and characteristic organs, such as mantle and ctenidium, which have been regarded as homologous throughout the Molluscan series, are interpreted in different ways in the different types, as the exigencies of Thiele's theory demand. One of the first propositions assumed by this writer is that the foot of the Mollusca is simply a colossal enlargement of the ventral sucker of the Polyclad; the suctorial function of the foot in *Chiton* and the lower Gastropoda is pointed to in support of this comparison. A series of more revolutionary propositions is then promulgated in consequence of the necessity under which the author is placed of discovering the primitive body-edge of the Mollusca comparable to the edge of the body of the Turbellaria. This primitive body-edge Thiele identifies by means of the lateral sense-organs which characterise the epipodium in the Rhipidoglossa and the margin of the mantle in Pelecypoda. The epipodium in Gastropoda and the mantle edge in Pelecypoda are thus taken by this writer to represent the sides or edge of the body in the Turbellarian ancestor. The epipodium in Gastropoda and the mantle edge in Pelecypoda consequently separate the dorsal from the ventral regions of the body in those groups. It follows from this that the ctenidia of Gastropoda, which are supra-epipodial in position, are not homologous with the ctenidia of Pelecypoda, which are infra-pallial. How we are to regard the anus, which is dorsal in the one group and ventral in the other, is not explained. But since in operculate Rhipidoglossa the operculum, like the shell, is situated above the epipodium, we are told that the operculum must also be regarded as dorsal in position, as well as serially homologous with the shell proper. This, in Thiele's eyes, compares well with the condition of affairs in *Chiton*, whose shelly plates are without doubt serially homologous. Moreover, although the existence of an epipodium in *Chiton* has not been hitherto recognised, Thiele argues that, since the pallial fold in this form represents the primitive body-edge, it must also, together with the series of ctenidia which are attached to its lower surface, be regarded as the homologue of the epipodium of the

Rhipidoglossa. The ctenidia of *Chiton* are, in fact, regarded as modified epipodial cirri. The consequence of this view is that while the mantle of *Chiton* and the mantle of Pelecypoda are regarded as homologous, the mantle of the Gastropoda is supposed to represent only a portion of the mantle in these other forms, and its projecting rim, similar as it appears to be in the two cases, is held to be a new and secondary formation unrepresented in the Amphineura and Pelecypoda.

Nowhere, however, do we find in Thiele's voluminous writings any explanation of the anomaly which ought to have occurred to him, that while in *Chiton* the anus is "ventral," and lies well beneath the "epipodium" and the last shell-plate, in operculate Gastropods the intestine opens not only above the epipodium, but between the operculum and the shell of the embryo—a relation which could only be represented in *Chiton*, if Thiele's theories were correct, by the situation of the anus between two of the shell-plates upon the back of that animal!

The nervous system of the Mollusca is treated by Thiele with a ruthlessness no less than that which is meted out to the external organs of the body. Let us take the Amphineura first. In this group, if the relations of the nervous system in *Chiton* be taken as typical, we have dorsal to the gut a great ganglionic nerve-ring whose lateral components are usually referred to as the lateral or pleuro-visceral cords. Connected anteriorly with the cerebral enlargements of this nerve-ring is a pair of ventral or pedal cords, connected with one another by a series of commissures lying beneath the gut, and also with the lateral cords by means of lateral connectives. The lateral cords innervate the pallial sense-organs, gills, and viscera; the ventral cords the musculature of the foot. The lateral cords are regarded by Thiele as the homologues of the lateral cords or nerve-ring of the Turbellarians, and the ventral cords are taken to correspond to the ventral longitudinal nerves of the same forms. So far we find nothing either erratic or original, for the same view has already been taken by Lang (16).

But the novelties begin with Thiele's interpretations of the nervous system of Gastropoda and Pelecypoda. We have already pointed out Thiele's view that the epipodium of Gastropods represents the primitive body-edge. Now at the base of the epipodium in *Fissurella* and *Haliotis* there lies a ganglionic plexus ; and this plexus, which takes the form of an incomplete ring, is regarded as the homologue of the lateral cords of Turbellarians and Amphineura. The series of epipodial nerves which connect the epipodial plexus with the upper half of the pedal cords in Rhipidoglossa is compared with the series of connectives between the lateral and ventral cords in Amphineura.

This seems very plausible until one recollects (1) that, the epipodium being infra-rectal, the epipodial plexus is also infra-rectal and thus difficult to compare with the lateral cords of Amphineura, whose "commissure" is supra-rectal ; and (2) that, whereas in Amphineura the lateral cords innervate practically the whole of the pallium and viscera, in Rhipidoglossa the epipodial plexus has nothing to do with any other organs except the sense-organs of the epipodium. If the pallium of the Gastropoda is really, as Thiele maintains, a secondary differentiation of the primary pallium of the Amphineura, one would expect that its innervation would also be effected by progressive differentiation of the nerve-centres which supplied the primary pallium, *viz.*, from the lateral or epipodial centres. So far from this being the case, however, Thiele himself (xxv., pp. 587-9) adopts the view that the pallial nerves as well as the pleural ganglia of Gastropoda are secondary derivatives of the ventral or pedal cords.

The recklessness of Thiele's comparisons reaches its high-water mark, perhaps, in his remarks on the nervous system of Pelecypoda. Correlated with the existence of numerous sense-organs (eyes, tentacles, etc.) along the mantle edge, there exists in many forms (*Arca*, *Pecten*, *Pinna*, etc.) a nervous ring around the mantle which may take the form either of a complete ring of peripheral ganglia united by a plexus, or of a circumpallial ganglionated nerve, as was recognised by Duvernoy (5) more than thirty years

ago. Since the mantle-lappets of the two sides of the body unite posteriorly above the anus, this pallial nerve-ring lies above the gut. The ring is connected with the cerebro-pleural ganglia by means of the anterior pallial nerves, and with the visceral (parieto-splanchnic) by means of branches from the great posterior pallial nerves. Accordingly Thiele homologises the circumpallial nerve-ring with the lateral cords of *Chiton* and with the epipodial plexus of the Rhipidoglossa.

The first of these homologies seems not unreasonable, for no one disputes the homology between the mantle of *Chiton* and that of Pelecypoda. Moreover Kowalevsky's discovery that *Chiton* in its later embryonic phases is provided with a pair of transitory eyes which lie outside the velar area and have some close connection with the lateral nerve-cords, renders this comparison particularly worthy of attention. But how the circumpallial nerve of Pelecypoda can be in any sense homologous with the epipodial plexus of Gastropoda, when the latter structure lies beneath the gut and has no connection with the cerebral ganglia, either directly or by the intermediation of the pleural ganglia, it is altogether impossible to conceive. And this is not all. The posterior connection between the circumpallial nerve of Pelecypoda and the visceral ganglia is compared by Thiele with the posterior connectives between the lateral and ventral cords of Amphineura; and the time-honoured visceral nerve-cords of Pelecypoda, with the visceral (parieto-splanchnic) ganglia upon them, are homologised with the ventral cords of the Amphineura. To reveal the absurdity of these comparisons it is sufficient, I think, to remind my readers that the ventral cords of *Chiton* are concerned exclusively with the innervation of the musculature of the foot; while the visceral cords of Pelecypoda innervate the body-wall, ctenidia and viscera, in addition to the posterior adductor muscle. How these supposed homologues of the ventral cords of *Chiton* have come to assume so many of the functions of the lateral or pallio-visceral cords, is not explained; and since Pelecypoda possess a pair of pedal ganglia in the foot, as typical in their relations

as those of any Gastropod—in *Nucula* to the extent even of having separate cerebro-pedal and pleuro-pedal connectives (18, 19)—it seems profitless to pursue these ill-balanced speculations any further.

The utmost ingenuity cannot overcome the fact that there is a fundamental disparity between the Turbellarian and Molluscan body. This disparity is revealed by embryology; but to embryology Thiele pays scant attention. Thiele's argument is practically this (24, p. 504),—that the only route from Cœlenterates to Bilateralia is *viâ* the Ctenophores to Polyclads, and that Annelids and Molluscs are consequently to be derived from Polyclad ancestors. Embryology seems to me, however, to point to two lines of descent at least, from the Cœlenterates to the Bilateralia. In each case the oral surface of the Cœlenterate ancestor became the ventral surface of the Bilateral descendant; but along one line of descent the primitive mouth or blastopore retained its ancestral form as a simple circular orifice in the middle of the ventral surface, and opened into a gastral cavity devoid of an anal orifice (Polyclads); while along the line of descent which led to the Annelida and Mollusca the blastopore elongated along the ventral surface, as Sedgwick has so ably contended, its lips coalesced except at the two extremities, and these open ends constituted the mouth and anus of the Cœlomate descendants. Thiele has altogether overlooked the significant behaviour of the blastopore in Annelidan and Molluscan embryos; and since no similar modification of the blastopore is known in the case of Turbellarians and Trematodes, in which groups the absence of an anus is so marked a characteristic, we are amply warranted, I think, in drawing the conclusions which I have emphasised above.

The admission of this distinction is however fatal to any theory of the Polyclad ancestry of the Mollusca. The foot of the Mollusca is a development of the fused lips of the elongated blastopore, and can in no case be homologised with the ventral sucker of Turbellarians which lies entirely behind the blastopore. The same remark applies to Lang's comparison of the Molluscan foot with the ventral

surface of the Turbellarian. The foot is undoubtedly part of the ventral surface of the Mollusc, and as such may be compared, in a general way, with the creeping surface of a Planarian; but as a specialised organ, developed from the fused lateral margins of a slit-like blastopore, it has no homologue in the organisation of the Turbellaria.

Let us now see what light has been thrown on the problems of Molluscan morphology by the researches of other investigators.

*The visceral commissure.*—One of the greatest difficulties in comparing the Amphineura with the Gastropoda or other Molluscan types has long been the fact that the lateral or pleuro-visceral cords of *Chiton*, which innervate the gills, viscera, and mantle, are united to one another posteriorly by a "commissure" lying above the rectum; whereas the visceral commissure of Gastropoda and Pelecypoda, etc., lies below the intestine.

A little care in the use of words would have prevented much of the confusion and controversy which has arisen on this subject of the position of the visceral commissure. Words, as Bacon phrases it, put constraint upon the intellect, and there is no doubt that the disagreement and perplexity of naturalists concerning this point have been caused by one of the *idola fori* which they have themselves set up, rather than by any intrinsic incompatibility in the facts themselves. If the language must still be maintained, I must at least point out that there are commissures and commissures, and that one may be a commissure in fact, and another only in name. The suprarectal "commissure" in Amphineura is ganglionic, and, like the rest of the pleuro-visceral nerve-ring, is formed *in situ* by delamination from the ectoderm (15). It is not a commissure in the strict sense of the word, but an integral portion of an annular central nervous system. But the visceral loop of other Molluscs consists merely of nerve-fibres connecting usually a couple of visceral ganglia with one another, and with the pleural ganglia. Now nerve-fibres are outgrowths from nerve-cells, and if two groups of nerve-cells should happen to take a somewhat deep-seated position in the body

before their fibres have grown out (which is not a rare embryological phenomenon), there should be nothing incomprehensible in their fibres taking the shortest route and meeting beneath the gut instead of over it. Clearly, therefore, the ventral position of the visceral commissure in most Mollusca by no means precludes the possibility of the essential homology between the visceral loop of these forms and part of the pleuro-visceral ring of Amphineura.

The other differences between the visceral loop of most Mollusca and the pleuro-visceral ring of Amphineura are principally differences in the degree of segregation and concentration of ganglion-cells and nerve-fibres. The pleuro-visceral ring of *Chiton* represents a very primitive nervous system, characterised by the more or less even diffusion of ganglion-cells over the whole length of the cord, while the nerves arising from it are not united into large trunks, but are given off at repeated intervals in a manner which is almost metameric. The nerves springing from it innervate the same parts of the body as the combined pleural and visceral ganglia of Gastropods and other Molluscs, *viz.*, mantle, ctenidia, intestine, heart, nephridia, and gonads. But if, after the reduction of the ctenidia to a single pair, we imagine a process of segregation to set in between these various elements, the more strictly visceral centres would become separated from the superficial pallial centres, and would assume a deeper position in the body. The law of concentration would apply in this as in other cases of evolution of nervous systems (3), and the result of the whole process would be the differentiation of a visceral nervous system, consisting of ganglia and commissural fibres, out of the primitively mixed and diffuse pleuro-visceral system. If the primitive relations to the gut and ring-like form were retained at all, they would be retained, not necessarily by the visceral system, which has *ex hypothesi* undergone considerable changes, but by the pallial (= pleural) system, which has undergone no change, except possibly one of incipient concentration.

The position of the commissural fibres of the visceral ganglion in relation to the gut becomes a matter of sub-

ordinate importance if the evolution of the nervous system has proceeded upon these lines, as will be made evident later on. As a matter of fact the visceral commissure is situated below the gut—a relation which is possibly foreshadowed in *Chiton* by a connection beneath the gut of the two gastric nerves described by Haller (8).

Pelseneer (19) indeed goes so far as to identify these gastric nerves of *Chiton* with the visceral commissure of Gastropoda and Pelecypoda; but the considerations which I have emphasised above show that the typical visceral nerves and commissure have not yet arisen in the Amphineura; they do not arise, in fact, until the branchial, nephridial, genital and enteric branches of the primitive pallio-visceral cords are all united into one common trunk. There is some doubt, moreover, as to the existence of the gastric nerves described by Haller, since two investigators, Plate (20) and Thiele, have been unable to discover them in species of *Chiton* examined by themselves.

A valuable contribution to this part of the subject is contained in Haller's recent *Studien* (11). In the common cyclobranchiate types of Limpet the pallial nerves are separate from one another behind, and seem to be mere outgrowths of the pleural ganglia (Bouvier, 3, p. 19); but in *Lottia*, one of the more primitive monobranchiate forms, Haller shows that the pallial nerves of the two sides are directly continuous with one another posteriorly, and make a complete arch round the edge of the mantle. They are moreover not mere nerves, since they consist of a core of fibres surrounded by an outer coating—discontinuous, it is true—of ganglion-cells. They are clearly the posterior continuations of the pleural ganglia, and represent the remainder of the pallio-visceral nerve-ring of the Amphineura after the separation of the visceral elements. This view is further borne out by the existence of several connectives between the pallial ring and the pedal cords in addition to the stout ganglionic connective which in higher forms becomes the persistent pleuro-pedal connective.

*The pleural ganglion.*—Haller's discovery recorded in the preceding paragraph shows clearly the error of the

view by which the pleural ganglion is regarded as a derivative of the pedal cords (Bouvier, Pelseneer, etc., *passim*). This view is founded on the fact that in the lower Gastropoda (Docoglossa and Rhipidoglossa) the pleural ganglia are directly continuous with the anterior ends of the pedal cords, while in the higher types the pleural ganglia gradually move further and further away from the pedal ganglia, and, travelling along the cerebro-pleural connectives, eventually come into contiguity with the cerebral ganglia (Tenioglossa) or even fuse with them to form a single cerebro-pleural ganglion on each side (Pelecypoda).

The close connection between the pleural and pedal ganglia in the lower forms may now be interpreted in a different manner. The ganglion-cells which were primitively distributed over the whole extent of the pallial nerve-ring have been concentrated at the anterior extremities of its lateral portions, as Haller's observations on *Lottia* show—or rather in the region of the first pleuro-pedal connective, for the most anterior portion of the primitive pallial cords is represented by the cerebro-pleural connective. The shortness of the pleuro-pedal connecting piece and the great concentration of ganglion-cells which takes place at its two extremities prevent any sharp demarcation between the pleural and pedal ganglia in these lower forms; but a comparison of the nervous system of *Lottia* with that of *Chiton* (Thiele, 23, p. 387) leaves no room for doubt as to the correctness of this interpretation, which throws a flood of light upon numerous other points which have been difficult to understand upon the older views. It explains, for example, why the cerebro-pleural and cerebro-pedal connectives should be already distinct from each other in the lower Gastropods at a stage when the pleural ganglia are in actual continuity with the pedal cords, and it sets at rest the controversy as to the meaning of the lateral furrow in the pedal cords of Rhipidoglossa which has been waged with so much skill in the rival pages of the *Archives de Zoologie* and the *Bulletin Scientifique de la France et de la Belgique.*

*Development of the pleural ganglion.*—That the pleural ganglion is essentially distinct from the pedal is, I think, sufficiently clear from the facts of development. Although these ganglia are placed so close together and are so intimately connected in the lower Gastropods there is not a single case on record in which the pleural ganglion has been observed to arise from the pedal ganglion, or from a common pleuro-pedal rudiment in the embryo. It is equally true on the other hand that Sarasin's derivation of the cerebral and pleural ganglia from a common rudiment in *Bithynia* (the cephalic sense-plate) has been opposed by v. Erlanger, who shows that all the great ganglionic centres arise separately, and do not become connected with one another until after their differentiation (7).

A renewed investigation of the origin of the cerebro-pleural ganglion in Pelecypoda would be of great interest in this connection. Pelseneer's (18) observations on *Nucula* have placed the fact of the composite nature of this ganglion in Pelecypoda beyond all doubt; and still, to the best of my knowledge, no one has yet observed the appearance in the embryo of a pleural element distinct from the main body of the ganglion. This apparent community of origin of the cerebral and pleural ganglia in Pelecypoda may be compared with the direct continuity of the cerebral and pleural elements of the nervous system in Amphineura.

*Development of the visceral ganglia.* — Sarasin endeavoured to show that the visceral ganglia of *Bithynia*, together with the pedal and abdominal ganglia, arise in the embryo from a common ventral proliferation of the ectoderm which he compares with the ventral ganglionic chain of Annelida. On this point also Sarasin has been corrected by v. Erlanger, who shows that all these ganglia arise separately from one another in *Bithynia* (7), as well as in *Paludina* (6).

The visceral ganglia are also quite distinct from the pleural ganglia in their origin, as v. Erlanger's observations show. In one important respect, however, the visceral ganglia and the pleural ganglia betray a marked similarity, the significance of which seems, however, to have escaped

the attention of its discoverer. In *Paludina* v. Erlanger figures the pleural ganglia arising from the ectoderm on each side of the body at a point just outside the velar area, but in actual contiguity with the cells of the ciliated ring. In *Bithynia* (7, Taf. xxvi., fig. 16) he figures the same condition of things for the pair of visceral ganglia. The only difference in origin between the two ganglia is that the visceral ganglia arise behind the pleural ganglia. If the Molluscan veliger possessed a nerve-ring beneath its prototroch (velum), as occurs in the trochosphere of the Annelida, it is quite clear that the pleural and visceral ganglia of *Bithynia* and *Paludina* would represent a series of ganglionic thickenings along the course of the nerve-ring. Apart from this inference, however, the topographical relations to which I have called attention seem sufficient to establish the proposition that the pleural and visceral ganglia, and, as I shall show directly, the abdominal ganglion also, of Gastropods—and, therefore, of other Mollusca—belong to a group of dorso-lateral nerve-centres quite distinct from that which is represented by the ventral or pedal cords. Here again we are reminded of the direct continuity of the pleural and visceral nerve-centres in the Amphineura.

*Development of the abdominal ganglion.*—In *Chiton*, as Kowalevsky has shown (15), the unpaired abdominal ganglion, or, as it is often called, the visceral ganglion, arises by a proliferation of the ectoderm at the hinder pole of the embryo, dorsally to the site of the future proctodæum. In the adult this ganglion is simply a special concentration of ganglion-cells on the supra-anal portion of the pleuro-visceral ring.

The abdominal ganglion of Gastropods is also situated at the hinder end of the visceral loop, but lies of course ventral to the gut. Can these two ganglia be regarded as homologous?

If Molluscs were mere mechanical models the answer would be undoubtedly in the negative; but embryology points unhesitatingly to the opposite conclusion. Von Erlanger has shown that in *Bithynia* as well as in *Paludina* the abdominal ganglion develops as an ectodermal pro-

liferation of the floor of the mantle-cavity, *i.e.*, that the ganglion is essentially a dorsal ganglion. Its final situation on the course of the sub-intestinal nerve-loop is rendered possible by the fact that its connectives with the visceral ganglia are not delaminated from the ectoderm, as are the ganglionic pleuro-visceral cords of *Chiton*, but are mere fibrous outgrowths from the ganglia themselves. Embryology is thus in complete accord with the views which have been maintained in the earlier part of this paper as to the homologies and origin of the visceral nervous system in Mollusca.

*The pallial and visceral commissures in Cephalopoda.*—It has long been known (Hancock) that in many Cephalopoda the stellate ganglia on the pallial nerve-cords are connected with one another above the gut by a transverse commissure. Is this commissure a relic of the pallio-visceral nerve-ring of the Amphineura and homologous with the pallial ring of *Lottia*, or is it merely a secondary connection?

In *Spirula* a remarkable arrangement of the pallial commissure has been recognised by Huxley and Pelseneer in their recent memoir (12). The commissure is not in this case a straight transverse band, but consists of two curved cords which arise from the right and left stellate ganglia respectively, and at their junction in the median line of the body give off a median pallial nerve which runs for a short distance forwards, and then passing over the anterior margin of the shell—which is, of course, internal—becomes recurrent and runs along the part of the mantle contained within the last chamber of the shell. Pelseneer is thus led to regard the commissure with its median nerve as formed by the two original pallial nerves fused together. The connection between the stellate ganglia having thus arisen in the primitive Dibranchiates (apparently in connection with the reduction in size and enclosure of the chambered shell), higher forms show a series of stages in its subsequent degradation, until it is finally lost in the Octopoda. The absence of a pallial commissure in *Nautilus* also supports Pelseneer's view that in Cephalopoda this structure is not of any primary importance.

At the same time when Pelseneer added a paragraph to the effect that the supra-rectal commissure of the Amphineura is also a merely secondary junction of the pallial nerves, he was probably not yet acquainted with Haller's work on *Lottia*, and allowed his views upon the Polychæte ancestry of the Mollusca to bias his interpretation of the Molluscan nervous system.

In a recent paper on the anatomy of *Nautilus* Mr. Graham Kerr (13) also refers to the question of the supra-rectal commissure. It will be remembered that in *Nautilus* the pleuro-visceral ganglia of the two sides form a stout ganglionic band encircling the œsophagus in the region of the cerebral ganglia. The pallial nerves radiate from the lateral portions of this half-ring, and the pair of visceral nerves arise from the ventral portion. The visceral cords pass backwards on either side of the vena cava, and, after giving off the branchial nerves, are prolonged posteriorly as far as the post-anal papilla, behind which Mr. Kerr has recognised an apparent anastomosis. Mr. Kerr adds that in this case "the homologue of the pleuro-visceral cord of *Chiton* is not merely the posterior sub-œsophageal nerve-mass, but rather the two lateral portions of this, together with the post-branchial prolongations which run on either side of the vena cava. The mesial part of the posterior sub-œsophageal nerve-mass would therefore be a secondary fusion between the nerve-masses of the two opposite sides."

In his suggested homology of this possible post-anal (*i.e.*, supra-rectal) commissure of the visceral nerves in *Nautilus* with the supra-rectal "commissure" of *Chiton*, Mr. Kerr has undoubtedly failed to appreciate the true nature of the posterior sub-œsophageal loop of *Nautilus*, as well as the relation of the visceral nerves to the pleuro-visceral cords of *Chiton*. The explanation of the Cephalopod nervous system is most readily found by comparing it with that of *Dentalium*, whose organisation in many respects supplies connecting links between that of the Cephalopoda and that of the primitive præ-torsional Gastropod or primitive Pelecypod. In *Dentalium* (22, p. 401) we find

a pair of post-anal prolongations of the visceral nerves precisely resembling those described by Kerr in *Nautilus;* yet in *Dentalium*, owing to the smaller degree of concentration or cephalisation which has taken place in the nervous system, it is easy to see that the typical sub-intestinal visceral commissure exists as in Gastropods and Pelecypods. The posterior sub-œsophageal nerve-mass of Cephalopods has clearly been produced, not, as Mr. Kerr suggests, by a secondary fusion of the pleuro-visceral nerve-masses of the two opposite sides, but by a simple shortening of the visceral loop as it occurs in *Dentalium*. This would bring the visceral ganglia into continuity with the pleural ganglia and with one another,—a process of condensation with which we are already familiar in the Tenioglossa and the Euthyneura among Gastropoda.

It may here be mentioned that Willey's simultaneous account (26) of the visceral nerves of *Nautilus*, while confirming Mr. Kerr's observations as to the existence of post-anal prolongations of a pair of visceral nerves, differs from his statement as to their origin. Willey states that the nerves supplying the post-anal papilla arise independently from the sub-œsophageal visceral loop, although at their origin they are adjacent to the branchial nerves and for a large part of their course are actually contiguous with them. The significance of this separation is not remarked upon by Willey; but if the separation really exists it is certainly a difficulty in the way of his contention that the post-anal papilla represents an approximated posterior pair of branchial sense-organs, since the anterior osphradium and both gill-plumes are all innervated from the outer visceral nerve.

*Euthyneurism.*—Since the publication of Spengel's paper on the olfactory organ and nervous system of Mollusca, a division of the Gastropoda into two groups, the Streptoneura and the Euthyneura, has been generally adopted. This classification has been accepted, moreover, not merely as an expression of the anatomical facts concerning the condition of the visceral loop in the two groups, but as a classification of phylogenetic significance. It is to be in-

ferred that the two groups have been independently derived from a common type of archi-Gastropod, possessing an untwisted visceral loop—the Prosobranchs (Streptoneura) by the twisting of the loop, the Opisthobranchs and Pulmonates (Euthyneura) by the mere shortening and concentration of the untwisted loop. This view derives support from the fact that the persistent ctenidium retains its primitive position on the right side of the body in Opisthobranchs, while in Prosobranchs it shows a marked displacement and lies on the left side. Bouvier's observations on *Actæon* (= *Tornatella*), however, have completely altered the position of affairs. *Actæon* is a very primitive Opisthobranch, as may be inferred from the high development of its shell, the persistence of its operculum, and the absence of pleuropodial fins. Bouvier tell us (4) that *Actæon* resembles the Prosobranchs, not only in these points, but also in possessing a distinct twist of the visceral loop (streptoneurism, chiastoneurie). The ctenidium is innervated from a supra-intestinal ganglion, which lies on the left side of the body. We are accordingly led to the conclusion that the euthyneurous condition of Opisthobranchs and Pulmonates has not been directly inherited from the orthoneurous ancestors of the Gastropoda, but has been derived from a previously streptoneurous condition. In other words the Opisthobranchs and Pulmonates have descended from Prosobranch ancestors, and the right-sided position of the gill-plume in Opisthobranchs is not primitive, but the result of a secondary process of detorsion.

*Orthoneuroidism.*—Without going further into the matter it may also here be mentioned that the supra-intestinal commissure has been recently discovered in various species of *Nerita*, *Neritina*, and *Navicella* by Boutan (2), Bouvier (3*a*), and Haller (11)—a discovery which destroys the last refuge of orthoneurism in Prosobranchiate Gastropods. Streptoneurism may now be affirmed of all Prosobranchiate Gastropods.

*Origin of the Molluscan nervous system.*—The attempts of previous writers to explain the relations of the nervous system of Mollusca have been based almost exclusively

upon comparisons with the fully constituted nervous systems of such types as the Turbellaria and Annelida. With Thiele's theory of the Turbellarian ancestry of the Mollusca I have already dealt, and I do not propose to deal with the Annelidan hypothesis, since this theory cannot provide any satisfactory explanation of the high development of the pleuro-visceral nervous system of the Mollusca. Those authors who, like Thiele and Pelseneer, homologise both the pleural and pedal centres of the Mollusca with the ventral cords of Annelids, base their view upon the supposed origin of the pleural centres from the pedal cords. This derivation I have already shown in this article to be completely erroneous. Pelseneer's theory of the origin of the Mollusca from Polychæte ancestors (18*a*), and all theories which seek the origin of the Mollusca in the specialised representatives of any of the vermiform groups, may at once in my opinion be dismissed from consideration.

Apart from matters of minor importance it will, I think, be conceded that the following cardinal points in regard to the morphology of the Molluscan nervous system have been established by the facts and arguments which have been presented in this article :—

(1) That the pleural ganglia have not been derived by segregation from the ventral or pedal cords.

(2) That the pleural, visceral, and abdominal ganglia of Gastropoda form a group of dorsal nerve-centres—the two former owing to their differentiation in the immediate neighbourhood of the velum, and the latter owing to its differentiation from the mid-dorsal wall of the body (floor of mantle-cavity).

(3) That the dorso-lateral nerve-ring of Amphineura is primitive and is represented in other groups of Mollusca by both the pallial and visceral nerve loops, or their derivatives.

(4) That the sub-intestinal position of the visceral loop in all groups except the Amphineura is a secondary one, which has been rendered possible

only by the decentralisation of the primitive pleuro-visceral nervous system, and its separation into special ganglia and nerves, the latter being formed ontogenetically as fibrous outgrowths from the ganglionic centres.

Venturing now, in conclusion, upon more speculative ground, I believe that the embryonic relations, to which I have drawn attention, between the pleural and visceral ganglia and the ciliated band are of phylogenetic importance. It has long puzzled me that the larval forms (trochospheres) of two groups so closely allied as the Annelida and Mollusca, while presenting a close similarity in general structure, should differ so remarkably in regard to their nervous system. The Annelid trochosphere has a nerve-ring beneath its ciliated band, while the Molluscan trochosphere has none. In this respect the Molluscan trochosphere appears to be less primitive than that of the Annelida. The explanation of this now appears to me to be as follows. In the evolution of the Annelida the prototroch and nerve-ring remained for a long time unmodified, and did not share in the elongation of the postero-ventral region of the body which gave rise to the trunk of the Annelid. This would explain the absence of the dorsal nerve-ring in the adult Annelid, provided that the nerve-ring, together with the prototroch, came to have merely a larval significance,—as actually happens in the ontogeny of Annelids to-day. On the other hand, in the evolution of the Mollusca from the same simple type of ancestor, the whole body must have shared in the elongation—the prototroch and nerve-ring as well as the more ventrally placed parts of the body. This elongated nerve-ring I identify with the pleuro-visceral ring of Amphineura, although the phyletic connection between the nerve-ring and the ciliated band is inferred from the development of certain Gastropods rather than from the Amphineura themselves. As a larval adaptation for conveniences of natation I imagine that a separation became gradually effected in embryonic life between the ciliated ring and the nerve-ring, the former becoming restricted to the anterior end of the larval body,

while the latter became more and more extended *pari passu* with the elongation of the trunk. Such a separation is to some extent paralleled in the development of Holothurians from the *Auricularia* larva, as described by Semon. On this theory alone can I explain to myself the absence of the ancestral nerve-ring in the trochospheres of Mollusca, and I find some support for this view in the ontogeny of Nemertines. The lateral nerve-cords in this group have the same relation to the gut and brain as have the pleuro-visceral cords of *Chiton*, since they form a dorso-lateral ring, the posterior commissural portion passing above the rectum. In Nemertines there can be very little doubt that this nerve-ring has been derived phyletically by the elongation of a nerve-ring which underlay the ciliated band of a more or less *Pilidium*-like ancestor, as it underlies the ciliated band of the *Pilidium*-larva, although this phyletic origin is disguised by the profound metamorphosis which breaks the continuity of the ontogenetic record in Nemertines. On this theory of course the lateral cords of Nemertines do not correspond to the ventral cords of Annelids. The latter are represented by the general ventral plexus of Nemertines and by the pedal plexus or cords of Mollusca. These ventral nervous systems appear to bear relations to the dorso-lateral ring-nerve similar to those of the subumbrellar plexus of Medusæ to the circumferential nerve-ring.

It will be recognised from these remarks that the conclusions to which I have arrived present distinct points of agreement with those of Balfour (1, p. 378) and Sedgwick (21) on the same subject, although attained throughout by an independent series of inductions. With both these writers I agree in tracing back the Molluscan nervous system to a primitively annular type, such as might be expected to exist in a Cœlenterate ancestor. Balfour derives the whole Molluscan nervous system from a peripheral nerve-ring which followed the course of a hypothetical ciliated ring; Sedgwick derives it from a broad plexus surrounding an elongated blastopore, such as occurs in existing Actinians. Sedgwick's theory was practically an alternative to Balfour's, but I find myself able to give a

partial acceptance to both these views. For the nervous system of Mollusca appears to me to consist of two parts, a circumferential ring and a peri-blastoporal plexus. The circumferential ring, which was primitively associated with a ciliated ring, is represented by the pleuro-visceral nervous system, which I have shown to possess significant relations with the velum or prototroch of the larva; and the peri-blastoporal plexus seems to me to be recognisable in the pedal nervous system, which in primitive Molluscs has a very diffuse plexus-like arrangement, and in Amphineura, at any rate, reveals its peri-blastoporal character in the cerebro-pedal connectives in front and its connectives with the supra-rectal abdominal ganglion behind.

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# THE RESERVE MATERIALS OF PLANTS.

(*Concluded.*)

THE position of the glucosides in vegetable metabolism has been for a long time a subject of considerable controversy, which has, however, been most largely concerned with tannin. The details of its formation, its localisation and its fate have been discussed at great length, but the discussion has been largely conducted on the lines of hypothesis and analogy rather than experiment. The conclusions reached by such a method of treatment have somewhat hastily been applied to all glucosides, as if tannin were eminently the typical one. There are now reasons for thinking that so far from this being the case it is especially exceptional.

The number of glucosides known has increased considerably in recent years as our investigations into plant metabolism have been pursued, and increasing knowledge of them forces the conviction more and more upon us that they take a more or less active share in the nutritive processes, possibly direct, but more probably through certain of the products to which they give rise on decomposition. They are not so markedly reserve stores for seeds as are many of the bodies we have already discussed, though many seeds, and notably many of those of plants of the Rosaceæ and Cruciferæ and orders allied to these, contain them in quantity together with other reserves. They occur, however, in other parts of the plant, not quite as circulating reserves, but rather as transitory stores for more localised growth and nourishment. The old advocates of their nutritive functions rested their case largely on the presence of sugar in the glucoside molecule, and held that this is the body which is available for the constructive processes of the organism. There are, however, reasons for holding that this view is too limited a one, and that some of the other products of their decomposition may be as valuable as the sugar, if not of even greater importance.

The glucosides that have attracted most attention during recent years are those which occur in the plants belonging to the families already mentioned, the Rosaceæ, the Cruciferæ, and other orders which show affinities with these. These plants contain, very widely distributed through their tissues, *amygdalin* and *sinigrine* or myronate of potash respectively. Of these the former is perhaps the most interesting, as from its decomposition by enzyme agency there is produced hydrocyanic acid, which has always been regarded as most virulent in its action upon all living things. The existence of this noxious principle in the plant has perhaps been partly the cause of the readiness of botanists to class the glucoside which yields it, and hence the whole class of glucosides, among the products of excretion.

The localisation of the amygdalin is calculated to throw a good deal of light upon the question of its probable function and fate. For many years attention has been given to it, at first, owing to imperfect methods of research, without much practical result. Improvement in technique has, however, yielded very valuable results, and has led to conclusions greatly at variance with those held thirty years ago. Thomé (60), who wrote in 1865 upon the nutritive materials contained in the sweet and bitter almonds respectively, said that amygdalin occurs in the parenchyma of the cotyledons of both varieties, and that its corresponding enzyme, emulsin, is only present in the bitter almond, being localised in the weak fibrovascular bundles that are in the cotyledons. This statement has been shown to be the exact converse of the truth. Portes (61), who worked twelve years later, showed that the glucoside and the enzyme occupy different parts of the seed, the former being distributed in the cotyledonary parenchyma, while the latter is to be found in the axis of the embryo. Pfeffer (62), in his *Pflanzenphysiologie*, suggests that this localisation is not accurate, and that the two bodies probably occupy the same cells, the only degree of separation being that the ferment is in the protoplasm and the glucoside dissolved in the cell-sap. In 1887 Johansen (63) by chemical methods succeeded in ascertaining the distribution of the

two bodies in the seeds. He found the emulsin to be present in both varieties of the almond, and to be chiefly localised in the fibrovascular bundles. He further ascertained that the glucoside, *amygdalin*, is only present in the cotyledonary parenchyma of the bitter one. The absence of the glucoside from the seed of the sweet almond points, of course, to the conclusion that even if it be a nutritive body it is not one of very great prominence in the nutrition of the embryo on germination.

Guignard has published within the past few years a series of researches which deal primarily with the localisation of the enzymes which decompose the glucosides, but which incidentally throw a certain light upon the occurrence and meaning of the latter. In his first papers (64) he treats of the amygdalin which is found in the almond and in the cherry laurel, in the latter of which it is found to have a fairly copious distribution. He confirms Johansen as to its position in the seed of the almond, and still more closely localises the enzyme. In the laurel (*Prunus lauro-cerasus*) the parenchyma of the leaves as well as of the axis appears to contain it in solution in the cell-sap. The occurrence of the emulsin is confined to the neighbourhood of the conducting tissues, it being chiefly found in the endodermis round the fibrovascular bundles. In the bundles of the axis of the embryo in the almond the ferment occurs in the many layered pericycle, chiefly outside the bast. The distribution of the amygdalin is not definitely known. It may happen that the fluid sap containing it may travel along the cellular tissue, and the occurrence of the ferment which decomposes it, in the immediate neighbourhood of the conducting tissues, suggests that it is charged with the duty of preparing from the glucoside certain nutritive products that may easily make their way to the conducting tissues, and so travel to the actual seats of constructive metabolism. That sugar so travels is of course a matter of every-day experience, but whether or no the remaining products are made use of in a similar way is open to discussion. On the other hand it may be that the amygdalin descends by the conducting tissue of the bast and undergoes decomposition as it passes downwards, yielding simpler products to the young cortex.

In the face of the problem of the utilisation of the bodies resulting from the action of emulsin upon amygdalin great importance must be ascribed to the recent work published by Treub on the occurrence and meaning of hydrocyanic acid in the tissues of *Pangium edule* (65), one of the Bixaceæ. This compound, according to the author, does not occur as a glucoside, but in the free condition, and is present in relatively large amount. Greshoff found more than 1 per cent. to be hydrocyanic acid of the dry weight of the plant in one sample among many others analysed. A brief *résumé* of the author's conclusions seems not to be out of place here, as throwing light upon the question of the nutritive value of the glucoside of the laurel. Indeed it seems not improbable that the hydrocyanic acid itself may be regarded as, in some cases at least, a reserve material.

Treub has made a careful investigation into the localisation of this principle in the plant, using as his method the reaction given in the formation of Prussian blue when hydrocyanic acid comes in contact with a ferric salt in the presence of hydrochloric acid. The reaction is very distinct and takes place well in the interior of the cells, causing those which contain the hydrocyanic acid to stand out with great distinctness.

In the whole of the adult axis, both stem, root and peduncles, he finds it to exist in quantity in the conducting tissue of the bast and pericycle. In the leaves it is still in the same regions, but is more widely spread, nearly all the parenchymatous tissue of the blade containing more or less of it. The epidermis especially is noteworthy, showing it present in the basal cells of the hairs which the leaves bear, and in certain idioblasts which contain also crystals of oxalate of lime. In the young fruits and those which are growing a considerable quantity is present, partly in the bast and partly in parenchyma outside the conducting tissue. In the seeds there is an accumulation in the peripheral layers of the endosperm and in other cells of the same tissue abutting on the embryo.

In these regions, and in the cortex, and sometimes the pith of the axis, Treub describes the hydrocyanic acid as

existing in special cells which are sharply marked off from the others round them when stained as above described. These special cells vary a good deal in number, apparently according to the amount of the acid present in the plant, and have no very specially regular distribution. Indeed it seems probable that any cell of the tissue may become a centre of deposition of the acid. Generally, if not quite isolated, they only occur two or three together. Certain of the fibres of the pericycle may be observed almost similarly isolated.

Treub further says that these special cells of the cortex or of the pith derive their supply of hydrocyanic acid from the conducting tissue of the bast and that the amount of them and consequently of the acid varies with the condition of the stem.

Tracing the hydrocyanic acid upwards through the axis by means of longitudinal sections it can be found to extend throughout its whole length, but to disappear at a little distance from the growing point, the apical meristem of which contains none.

It is impossible to avoid being struck with the similarity here exhibited to the fate of sugar, amides, etc., which as we have seen can be traced up to the seats of constructive metabolism and there cease, apparently giving rise to protoplasm. If this be so, the hydrocyanic acid must be regarded as a plastic material, unsuitable as at first sight it would appear for that purpose.

This view is supported by several observations which the author details at some length. He finds that in the apices of young shoots which have suffered an arrest of growth, there are more of the special cells containing the hydrocyanic acid than there are in similar ones which are undergoing rapid elongation. That is, where there is active consumption of plastic material there is no accumulation of the acid, but where plastic substances are compelled to remain unused, hydrocyanic acid is one of such stored bodies.

Another series of observations considerably strengthens this view, while it points more definitely to the ultimate purpose of the acid. In many of the special cells the latter

may be seen to be accompanied by quantities of proteid substance. Taking young cells near the apex of the shoot the special cells contain the hydrocyanic acid alone, showing that it precedes proteid in the time of its occurrence. A little farther back the proteid can be detected, and gradually as sections are taken at increasing distances from the apex it increases in amount while the acid diminishes. As the active life of the cells becomes less and less vigorous, the proteid becomes more and more preponderating in the cell contents, and ultimately cells are found which contain proteid only, the hydrocyanic acid having all disappeared. The same succession of events can be seen if the development of the pericyclic fibres be traced towards the apex of the stem.

There seems from these observations to be very strong reasons for supposing that hydrocyanic acid is a nutritive substance and leads at any rate in these plants to the formation of proteid.

Treub holds that this is its immediate function; he believes it to be primarily formed in the leaves, principally in the basal cells of the hairs and the idioblasts with calcic oxalate in the epidermis of the leaves. Thence it makes its way to the conducting tissues of the bast and pericycle and travels to the apical meristems. It is thus primarily a body originating only in the constructive processes, and not, as in the cases of the almond and cherry laurel, the product of a decomposition of a glucoside. Indeed Treub says very emphatically: "L'acide cyanhydrique du Pangium edule n'est *pas* un produit de decomposition ou de dèsassimilation," basing the statement on both indirect and direct arguments. The former are founded on the localisation of the product in the bast and pericycle and its evident transportation by the bast tissue. The latter involve the consideration of its localisation with a material which serves as a temporary proteid reserve in the same elements of the tissues, and the order of appearance and disappearance of the two substances in such special cells.

That hydrocyanic acid can subserve not only the formation of temporary reserves of proteid but can be used,

immediately after its first formation, by the leaves in which it is formed also appears certain. When plants whose leaves contain it are put for some days in the dark the acid gradually disappears, and as usual in such cases their whole metabolism suffers. On being again illuminated the vital processes gradually resume their activity. If a plant be put in the dark till nearly all the acid has gone from the leaves and then it be brought into the light, the little that remains is soon removed by the returning activity of the metabolism.

That the acid is used, and not simply transported from the leaves, can be shown in another way, by cutting a circular section through the conducting tissue of the petioles, when removal by transport becomes impossible. Yet the hydrocyanic acid disappears gradually.

It was said above that in some cases the hydrocyanic acid itself might be looked upon as a reserve material. This seems to be the case in the special cells described by Treub in the cortex of plants when they do not contain also proteid. In such cases we seem to have temporary reservoirs to supply local and transitory needs and to supplement the current passing along the bast. "Dans les endroits non on pas suffisamment desservir pour le système conducteur libérien ces usines locales prennent naissance, et en plus grand nombre, à mesure que la plante a on aura besoin dans ces endroits de plus de substances plastiques." Thus in the older part of the stem, where the active life is confined almost altogether to the cortex, the latter contains many of these special cells, while they are absent from the rest of the fundamental tissue. Where they are present, as in certain portions of the petioles, active life continues, although it may be decadent in other parts.

This temporary storage comes out very prominently in the cases of the developing fruit and seed. At the base of the former, just above its point of junction with the pedicel, there is a very marked accumulation of the hydrocyanic acid, the cells staining blue under the treatment described being much more numerous than lower down the stalk. The

peripheral layer of the seed in its young condition is also supplied very fully with these local reservoirs. We appear to have here a deposit laid down to supplement the regular stream which is passing all about the plant by means of the conducting tissue of the bast. It is doubtless derived from the circulating supply, for if the latter be interrupted by a section passing across the stem through its path, the disappearance of the acid takes place from the bast tissues below the wound some time before it does from the isolated special cells of the cortex.

From the work of Treub and of Guignard then it seems increasingly probable that the glucosides are reserve materials, and not simply bye-products or products of excretion. Nor is it apparently only the sugar in them which has a nutritive value, but the other products of their decomposition have a particular part to play in the metabolism. This is certainly the case with hydrocyanic acid, and no doubt further investigation will show that it is the same with other products similarly formed.

Guignard (66, 67) has made similar researches to those already described upon the plants of the natural orders Cruciferæ, Capparidaceæ, Tropœolaceæ, Limnanthaceæ, Resedaceæ and Papayaceæ; which all contain the ferment *myrosin*, a body capable of decomposing more than one glucoside. There are several of the latter compounds found in this group of plants, the best known of which are sinigrine, and sinalbine. Sinigrine is found in the black mustard (*Brassica nigra*), and is often called myronate of potassium. On decomposition it yields besides sugar a volatile body, sulphocyanate of Allyl, and potassic hydrogen sulphate. Sinalbine, as its name implies, is found in the white mustard (*Sinapis* or *Brassica alba*). When decomposed the volatile constituent is found to be sulphocyanate of orthoxybenzyl. Others, the composition of which is not yet fully known, are those of the watercress (*Nasturtium officinale*) which yields phenyl propionic nitrile, the common cress (*Lepidium sativum*) affording the nitrile of alphatoluic or phenylacetic acid. Though the fate of these complex volatile bodies has not been investigated, it is

noteworthy that some of them at any rate contain cyanogen compounds, which may well be utilised after the manner of hydrocyanic acid itself as established by Treub.

Their distribution in the plants appears to follow that of the amygdalin in the Rosaceous group, but very little definitely is known on this head. The enzyme which splits them up is according to Guignard always found in special cells which do not contain the glucoside.

Very closely allied to the group of the glucosides is that of the tannins, about the importance of which there has been a good deal of controversy. Some of them are no doubt glucosides, yielding among their products of decomposition gallic acid and sugar. Others are apparently not so associated with a carbohydrate group. They are very widely distributed, and often occur not only in parts of plants which are devoted to storage of materials, but in the tissues where active metabolic work is going on. The task of deciding whether or no they serve as reserve materials or as bye-products is consequently not easy.

The two views have been strenuously supported by different writers. Sachs, while working on the germination of the Scarlet-runner (68) in which tannin is comparatively plentiful, suggests an antithesis between carbohydrates and proteids on the one hand, and the tannins and colouring matters on the other, the latter being in his opinion only bye-products. He advances in support of his view the fact that they appear or increase with renewed growth of the embryo, instead of diminishing as reserve materials should do. Their appearance is coincident with the chemical changes in the undoubted reserves which lead to the utilisation of the latter. The same view is advanced by Schell (69), who suggests that in some cases, however, it may be a nutritive product. In the germination of certain oily seeds, chiefly of plants belonging to the Boroginaceæ, tannin, which is present in addition to the oil, diminishes in quantity during the germination. In the stem of the mature plant there is during the winter a considerable quantity of tannin which almost vanishes as spring advances. On the other hand he finds in certain almost parallel cases that the tannin accumulates instead of diminishing.

The view that these bodies have a nutritive value has been supported with some emphasis by other writers. Wigand associated it very closely with the carbohydrates, and thought it was an essential factor in vegetable metabolism. Wiesner also supported the view of its carbohydrate relationships, and indicated a probability that it stands between the starch and cellulose groups and the great class of resins, etc. The latter relationship has been again brought forward by Hillhouse (70), who found in *Pinus sylvestris* that as resin increases in the stem tannin diminishes in like proportion, and that the cells surrounding the resin ducts invariably show its presence. Hartig suggests that tannin remains in the oak through the winter in the form of grains similar to starch grains, but distinguishable from the latter by characteristic reactions. These grains, he says, are dissolved and utilised in the spring. In his later writings Sachs inclines to the same view; he says that besides those which must be looked upon as excreta or bye-products, some of the tannins of the oak are most likely to be regarded as reserve products, on account of their origin and disappearance and their behaviour generally during the growth of the plant (71).

The localisation of tannin in the different parts of the plant does not give us much assistance in determining which of these views has most to support it. It is often found in special sacs in the midst of metabolic tissues; it is very frequently found in epidermal cells, either in the interior or saturating the cell wall; it is extremely prominent in bark. These positions certainly suggest that it is of but little value as a food-stuff; on the other hand it is often abundant in assimilating parenchyma in which starch formation is proceeding.

In Hillhouse's paper (70) already alluded to, the author describes a considerable number of observations he made to determine whether or no a disappearance or diminution of tannin could be detected in the spring, and if so, whether it was a reasonable conclusion that such diminution indicated a utilisation of the vanished portion.

He investigated a large number of trees in which tannin

is present in greater or less amount, and noted the changes in the amount present in winter and in spring in their various tissues. He concludes that in no case is there noticeable a diminution of tannin in early winter as starch accumulates, and there is no sign that the starch is formed at the expense of the tannin. When growth recommences in the spring, instead of tannin disappearing from the older tissues it makes its appearance in quantity depending on the amount of growth. The tissues of the bud are commonly crowded with it. Hillhouse's experiments proceeded upon three lines. In the first place plants or parts of plants rich in tannin were made to grow under conditions in which assimilation of $CO_2$ was impossible; a second set of experiments consisted of germinating in darkness seeds containing tannin; and finally corms were investigated to see whether, as their nutritive material was transported to the newly-formed corm springing from them, tannin was transferred together with the starch.

In no case was any diminution or transference found, except in the case of *Pinus sylvestris* already alluded to, when the probability of the tannin being an antecedent of the resin became evident.

Those tannins which are undoubtedly glucosides must, however, be of some nutritive value, as they give off sugar on decomposition taking place. There is some evidence to show that during the ripening of certain fruits part of the sweetness is derived from an astringent principle resembling and probably identical with tannin, which diminishes in quantity as the fruit matures (72).

A similar uncertainty as to its physiological meaning must for the present be associated with phloroglucin and the compounds into which it enters, which are to be regarded as ethers corresponding to glucosides. There are two classes of these compounds, which have been described as phoroglucides and phloroglucosides respectively. The former include such bodies as hespentine, phloretine, etc., while the latter, which contain a sugar group in their formula, embrace aurantine, rhamnine, hesperidine, etc. They are somewhat difficult to localise, as the reactions they give

are either not well ascertained or not particularly distinctive. The most reliable is perhaps that with vanilin in the presence of hydrochloric acid. When this is made to react upon a cell which contains phloroglucin in the sap, the latter forms a fine precipitate of red granules which are composed of a compound of vanilin and phloroglucin, known as *phloroglucivanilin.*

Phloroglucin appears to be often present in the plasma of meristem cells rather than in the vacuole, for when chloride of vanilin is added to a tissue containing it the colouring mainly affects the protoplasm, some of the vacuoles remaining altogether uncoloured.

The distribution of phloroglucin, like that of tannin, leaves a good deal of uncertainty as to its physiological meaning. It has been investigated in recent years by Waage (73), who has carefully examined representative plants taken from almost all sections of the vegetable kingdom. Out of 185 plants submitted to experiment 135 showed it to be present, but in very different quantities. Of the 135, 51 contained a very considerable quantity, 41 less but still a tolerably large amount, while in 43 though present only a feeble reaction could be obtained. Its distribution was to a certain extent regular, for the author states that if one species contains it, it is found with tolerable certainty in all the species of that genus. The plants of the Polypetalæ as a rule show most, while the Gamopetalæ and the Monocotyledons are on the whole poor in it; lower down in the scale the Vascular Cryptogams and the Gymnosperms are charged with it to a degree intermediate between the other groups.

Examining the tissues of such plants as contain a considerable quantity it may be found in meristems and in permanent tissues. In axial organs it occurs in the epidermis and later in the bark; also in the parenchyma of the cortex, and in the sclerenchyma of the tissues more deeply seated. It is found sometimes in the endodermis; also in the dead cell walls of the xylem parenchyma, fibres, and vessels. The medullary rays frequently contain a certain quantity. It is uniformly absent from the bast

fibres and the sieve tubes, and may be present or not in the pith. When the epidermis contains it, it is usually in the hairs if any are present; even root-hairs giving evidence of a certain amount. Taking the members of the axis, Waage found that roots as a rule contain more than stems, unless the latter be rhizomes, in which it is fairly abundant. Petioles and the peduncles of flowers contain less than branches. In plants where the axis is highly charged with it, there is generally a quantity also recognisable in the leaves, chiefly occurring there at the edges near the endings of the veins, and further in the neighbourhood of the vessels of the latter. The palisade tissue of the leaf has usually more than the spongy mesophyll, and the upper has more than the lower epidermis. The seed as a rule contains but little, and that is only in the integuments.

If the disposition may be taken as any indication of its being a reserve material at all, the probability is that its value in the latter sense is but slight. The disposition of varying amounts in the medullary rays and its frequent presence in the cells of the cambium layer point possibly to its supplying nutritive material for the latter. On the other hand, its consistent absence from all parts of the seed except the integuments seems to indicate that storage of nutriment is not its main purpose. It may be that its value to the meristem tissues is based upon its easily oxidisable character, affording energy thereby, rather than being a reserve substance. Its occurrence in the leaves in the localities named suggests a formation in the mesophyll and a subsequent transport to the axial regions. But against the view of its value in metabolism as a reserve material we have the statement that light does not affect its formation. It is in Waage's opinion found in the cell-sap as a general rule, rather than in either protoplasm or choroplastids. It seems on the whole to be a product of destructive metabolism, for it occurs in the same cells as starch and sugar and may be derived from the latter by abstraction of three molecules of water, $C_6H_{12}O_6 - 3\ H_2O = C_6H_6O_3$. It seems to resemble tannin in that it often

increases with the greater development of the plant, and in being frequently plentiful in parts that are thrown off from the latter, such as old leaves, the coats of fruits, seeds, etc., and in regions withdrawn from active metabolism, such as bark and to a less degree epidermis. In a further paper Waage and Nickel suggest that it may possibly be a source of tannin, as the latter is generally found in the same parts as phloroglucin (74). Tannin does not appear, however, to give rise to phloroglucin.

Like tannin, therefore, phloroglucin appears to be on the whole an accessory product and only rarely to act as a reserve material. The compounds of it which contain sugar, *i.e.*, the phloroglucosides, may serve as such, yielding sugar on their decomposition.

In certain cases the alkaloids appear to serve as reserve materials, though their value in this direction is probably but slight. Many seeds which contain them in some considerable quantity lose them during germination, and other bodies, principally amides, replace them in the developing embryo or young seedling. This is especially the case with the seed of *Lathyrus Sativus*, an Indian species which contains sometimes as much as ·5 per cent. of its dry weight of an alkaloidal product known as *viciine* (75).

The possibility of alkaloids helping in such cases to form albuminoid materials or proteids has been pointed out by Jorissen (76) in his discussion of the chemical processes incident to germination, in which he claims for them a certain value as reserve materials. Heckel (77) comes to the same conclusion. He carried out experiments with *Sterculia acuminata*, *Strychnos Nux-vomica*, *Physostigma venenosum*, and *Datura Stramonium*, and found in all these cases that during germination the greater part of their alkaloidal principles disappears. He claims that this disappearance is due to a transformation into assimilable substances under the influence of the embryo. If the latter be extracted from the seeds, and they be then surrounded by or buried in moist earth, the alkaloids remain for a considerable time unchanged.

The conclusions of Jorissen and Heckel are disputed by

Clautriau (78), who finds another explanation of the disappearance of the alkaloids during germination in a possible destruction of them as deleterious bodies which would affect prejudicially the development of the young seedling. He has ascertained with considerable precision the distribution of the alkaloid in the seeds of *Atropa Belladona*, *Datura Stramonium*, and *Hyoscyamus Niger*, and states that it is confined entirely to a layer of cells situated between the albumen and the integument of the seed, which when the latter is mature is very much reduced in its dimensions. This layer is much more prominent while the seed is ripening, consisting of many cells with very rich contents, the latter consisting of starch and albuminoid substances as well as alkaloids. As the albumen grows, this nourishing layer gradually yields up both starch and proteids, while the alkaloid persists; the cells become gradually nearly empty, and dry up considerably, ultimately becoming dead. In this condition they still contain the alkaloid, the quantity of which does not diminish during the changes described. When the seed is mature, this layer is very thin, the cells being flattened and compressed together, forming a sort of membrane in which the alkaloids remain, partially or wholly combined with an organic acid.

The nutritive value of the alkaloid seems improbable when we consider the disappearance from this layer of the starch and proteids, and the retention of the former. If it were then a reserve product it would in all probability accompany the other undoubted nutritive bodies. Clautriau has obtained further information on this point by depriving seeds of *Datura Stramonium* of this alkaloidal layer and submitting them to germination, either in moist earth or in an atmosphere saturated with watery vapour. He found that under such conditions they germinated normally, and produced young seedlings which differed in no particulars from normal seedlings of *Datura*.

Clautriau extended his researches to other plants than those named, particularly *Conium maculatum*, from which he obtained the same results.

Examining the young seedlings grown under these

conditions, no alkaloid being allowed to remain in the seed, Clautriau found that the active principle made its appearance in considerable quantity, and chiefly in the growing apices. The same thing was noticeable in the development of morphine in the poppy (79), where a more gradual formation was detected. Morphine does not show itself at the outset of the development of the plant, but appears to be preceded by another alkaloid, giving very clear reactions, which does not seem to be identical with any of the nitrogenous principles extracted from opium.

The conclusion that must be drawn from these investigations is that these alkaloids, and hence probably all such bodies, are not to be regarded as reserve materials, but as bye-products or excreta, appearing coincidently with the active metabolic processes of the growing plant.

Besides these accumulations of more or less complex organic compounds in the tissues of plants we meet with certain cases where inorganic material is deposited with a view to subsequent utilisation. These are, however, of much less importance and only occur in comparatively few plants. We have the well-known globoids in the aleurone grains of the castor-oil seeds, the seeds of *Bertholletia excelsa* and several others. From their disposition and fate, and from the fact that they afford a supply of phosphorus, it is probable that we may include them in this group. In certain cases also the collections of crystals of calcium oxalate gradually disappear from the cells in which they are deposited, and so seem to minister to the needs of the plant for calcium, an element whose function, however, is still practically unknown.

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# AFRICAN GRASS FIRES AND THEIR EFFECTS.

MANY parts of the interior of tropical Africa consist of wide grassy plains, occasionally varied by scattered trees, but usually very bare and monotonous in appearance. In the rainy season these steppes are green with vigorously growing grass, and patrolled by hundreds of antelopes and other kinds of game; a few months afterwards when the rains are over, they are covered by blackened ashes and charcoal, and not a living creature will be visible except perhaps a few birds or a very occasional ground-rat.

These fires are usually due to the natives, who find that the bush can be most easily cleared by their assistance, though they are often lighted to satisfy the childish delight in a big blaze which is characteristic of the Suahili porter.

Their effects are most interesting, both economically and also in the way in which they entirely change the aspect of the vegetation.

It is, of course, immediately obvious that all the valuable feeding material of many square miles of luxuriant grass is by these fires entirely wasted; but, besides this, the soil is never permitted to grow rich through the accumulation of leaf-mould and stems, and in fact the land is every year brought back into exactly the same condition. No true turf is formed, and the soil remains more like the subsoil in cultivated countries and never becomes in the least improved.

The effect on the vegetation is very curious. The season of flowering for many trees and herbaceous plants is completely altered. A large number of low-growing herbaceous plants possess woody root-stocks or some sort of underground store of nourishment. With the very first shower of the rainy season, these stores send up flowering stems entirely without leaves, and the bare and blackened earth is studded with the bright purple flowers of

*Dolichos* spp., the blue *Pentanisia Schweinfurthii*, little white Euphorbias, *Lasiosiphon* spp., etc. These all have the appearance of a flower cut off and planted in the earth, and give rise to remarks on the collector's carelessness in not bringing leaves when worked up by untravelled botanists. With the setting in of the rains, the stems begin to grow and produce leaves until, when the grass has sprung up, all these herbs are in full foliage. This habit is of great advantage to the flowers concerned, as insects can readily perceive the scattered flowers which in the grass would be quite inconspicuous. The same thing occurs in many of the trees. Several species of *Dombeya*, for example, send out their flowers at this foreshadowing of the rains and are most conspicuous.

Another curious effect of the fires is the manner in which trees are either kept down or obliged to protect themselves in some way against their action. In the more arid plains trees seldom exist, or if present occur in the form of stumps perhaps ten years old, but never able to grow higher than a foot or so. Such stumps put out every wet season vigorous shoots, which are annually burnt away and only the short stem with another layer of wood is left to survive.

Of the trees which do manage to exist in spite of the annual conflagration, the most remarkable are the tree Euphorbias, often twenty to twenty-five feet high. These have angular fleshy branches protected by a leathery epidermis, and besides their milky juice, which contains gum, caoutchouc and other substances, have a large amount of mucilage or slimy matter in the ordinary tissue. This latter is a strongly waterholding substance, and the most violent fire seems unable to do more than scorch a very few of the outermost branches.

It is a most curious fact that though when living they resist fires in this wonderful manner, dead branches make an excellent fire and blaze up most vigorously. I cannot understand this difference.

Of the other trees which continue to thrive in these places, there are some seven species which grow in abun-

dance; there will be usually 500 of one of these species to every individual of some other kind. I brought home specimens of the bark of these six or seven forms, which were given to Professor Bretland Farmer for examination, who replied as follows: "I examined your specimens of bark and they all agree in possessing cells which show a certain amount of gummy degeneration of the cells in the bark, together with the presence of a considerable amount of sclerotic cells; it seems not impossible that these two facts may be connected with the resistance of the plants to the fires, and I found as a matter of fact that, on comparing the rate of burning of these barks with that of laburnum, they were very slowly consumed.

"I should have added that there are repeated periderms, and intermixed with the cork are the sclerotic cells already mentioned." Now the artificially produced cork of commerce shows great similarity in some respects to the cork of these fireproof trees. The process adopted both with the birch and the cork oak is to carefully peel off the cracked superficial layer of bark or "male cork" (this is known as "démasclage"). After this the layer of cork increases enormously and may perhaps attain to 17 cm. in thickness if left untouched: the result is the ordinary commercial article. I do not think that it is going too far to say that we have in grass fires a natural "démasclage" process, for they will certainly destroy the outer more or less dead tissues.

From the researches of Henslow,[1] Tschirch[2] and Volkens[3] on desert plants, it may be considered proved that cutin, which most modern authorities consider nearly identical with suberin, is directly increased by dry and arid conditions, so that this direct effect is probably also of use in increasing the deposition of corky matter. Both evils—the fire and the drought—have, as so often happens, brought about their own remedy. The sclerotic cells (or stone cork?) may doubtfully be set down to the same cause, for

[1] *Origin of Plant Structures.*
[2] *Angewandte Anatomie* and *Linnea,* 1881.
[3] *Flora der egypt. arab. Wuste.*

culture experiments (Duchartre and Henslow, *loc. cit.*, p. 57) show that sclerenchyma may be directly diminished by a more moist atmosphere.

The occurrence of gum is not so clearly dependent on the climatic conditions ; its use in these forms is, however, obvious enough, for all apertures by which water might be lost are, so to speak, gummed up. This is quite similar in physiological action to the drops of mucilage or gum which hermetically seal the vessels exposed by cutting across a branch of any ordinary deciduous tree.

It is true that the production of gum is known to be most abundant in a dry and hot season, but according to the explanation given by Tschirch, *loc. cit.*, p. 211 (and an identical account has been given me by Mr. Malcolm Dunn as the result of experience), this is due to the gum being squeezed out by the contraction of the bark following on a wet period, during which the masses of gum in the bark are greatly swollen. I cannot find any explanation of the actual cause of the change of cellulose into gum, but Mr. Malcolm Dunn states the general opinion that it is abundant after a severe shaking of the trees, as, for example, in a violent wind. Such places as those here treated of are certainly exposed to wind (otherwise they would be covered by forest, according, that is, to my experience), and it is possible that the wind may have assisted in starting gum formation ; but if, as is not unlikely, the wind acts indirectly by straining the layers of the cell walls, it seems more probable that the fierce heat of the fire, causing sudden and violent shrinking and warping of the bark, strains the cell walls in the same manner. This may of course be quite unproved, but the facts are sufficiently interesting to justify further research.

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# THE GENERAL BEARINGS OF MAGNETIC OBSERVATIONS.

IF necessity be the surest prompter of invention, it is not too much to say that the necessity of the navigator has been a most potent factor in producing the observer of the elements of Terrestrial Magnetism. The traveller on land might rest during darkness until daylight enabled him to resume his journey; but the seaman on the trackless ocean was dependent upon the indications of his compass by day and night; and after the discovery of Columbus that the magnetic Declination or Variation of the needle from the direction of the geographical North varied in amount with the Latitude and Longitude, a new impetus was given to observation.

The publication of Gilbert's grand discovery that the earth is a magnet and the director of the freely suspended needle, followed by the discovery of the secular change in the value of the Declination, naturally added to the desire of both landsmen and seamen to know as much as possible concerning that great magnet, both from purely scientific reasons and to meet the practical ends of the navigator. Thus the seventeenth and eighteenth centuries were remarkable for the number of observers both of the magnetic Dip and Declination.

So important had a correct knowledge of the Declination become to the requirements of navigation, as early as the close of the seventeenth century, that Halley, under the

immediate auspices of the Government, made his celebrated voyage over the Atlantic Oceans in a man-of-war, in order that intelligent observation should set at rest much that was doubtful. The results of this voyage, combined with the observations of previous navigators, were embodied in his celebrated chart of lines of equal value of magnetic Variation or Declination, the first of its kind and of so convenient a form that charts of equal values of the three magnetic elements are to this day the most acceptable form for representing the combined results of magnetic observations made over large areas of sea and land, as well as of the special magnetic surveys which in recent years have been made in various countries.

Here we may pause to consider the word Declination as applied to the angle which the direction of the horizontal magnetic needle makes with the true meridian. Many magneticians object to the word, but no better has yet been proposed or at any rate accepted; the result being that while observers on land use the term, seamen adhere firmly to the expression "Variation of the Compass". This is as might be expected when it is remembered that navigators look upon the word Declination as connected with the position of the sun and other heavenly bodies, and would find it most inconvenient to have the same word in daily use, meaning two totally different things.

During the eighteenth century charts of the magnetic Declination were published by Mountaine and Dodson, Bellin, and Churchman, and for their time may be considered as fair approximations to the truth. Churchman's design was not only to give values of the Declination but to furnish the seaman with a means of ascertaining the Longitude, an ambitious project, especially as we now know there were probably considerable elements of error in these charts caused by local magnetic disturbance of the observing compass on land, and from the iron used in construction disturbing the compass on board the ships.

This latter source of error was only beginning to be viewed in its true light at the close of the eighteenth century.

In the years 1801-2 Commander Flinders of H.M.S. *Investigator*, then surveying the southern coasts of Australia, found that when his vessel's head was north or south by compass the observed Declination agreed very nearly, but when she lay with her head east or west, it differed largely. Moreover these errors on the east and west points of the compass had the opposite sign to those observed in England.

Flinders, however, had supplemented the existing scanty knowledge of the distribution of the Dip over navigable waters by several observations of his own in northern and southern latitudes, and from these he drew the conclusion that the errors in the Declination observed on board ship were caused by magnetism induced by the earth in the vertical iron of the ship, and changed in value proportionally to change of Dip. Here Flinders was wrong, as the errors were really proportional to the tangent of the Dip.

In spite of this mistake he was enabled from his knowledge of the Dip to conceive the idea of so placing vertical bars of iron that they produced an equal and opposite effect on the compass to that of the ship in all latitudes, and thus invented what is now called the Flinders bar, one of the most important correctors of compass disturbance in the iron and steel ships of the present day.

In 1814 Flinders induced the Admiralty to have experiments made on board men-of-war at Portsmouth, Sheerness, and Devonport, to ascertain the amount of the magnetic disturbance of the compass caused by the iron in each ship. The chief reason for making these experiments was to show the necessity for ascertaining and applying these errors to ensure the safe navigation of the ships, but it had also a direct bearing in enabling observers to eliminate the hitherto inexplicable divergencies in the values of the Declination observed in different ships in the same geographical position. The results of these experiments bore no immediate fruit, for with the death of Flinders the subject was temporarily neglected.

In 1819, Hansteen published his *Magnetismus der Erde*

with an atlas containing charts of the elements Declination and Dip for different epochs between the years 1600 and 1787. These charts were in a large measure compiled from observations made with imperfect instruments and subject to the causes of error already mentioned attending both land and sea results. Hansteen, however, considered them of sufficient value to enable him to draw certain important conclusions with regard to the cause of the secular change of the magnetic elements. Thus he not only concurred with Halley that the earth considered as a magnet had four poles or points of attraction, but computed their geographical positions. Further than this, he computed that to account for the secular change these four supposed poles revolved round the terrestrial poles, each pole occupying a widely different number of years to complete the revolution.

If these theoretical results had been true, a great advance would have been made not only in the science of terrestrial magnetism but in its practical bearing on the requirements of the present day.

Although Humboldt had about the year 1800 shown that the intensity of the earth's magnetism varied with the latitude, the general distribution of that magnetic element was so little known that we may with our present extended knowledge consider that Hansteen's conclusions were based on insufficient data. In fact the idea of the earth being a magnet with four poles has long since been abandoned in favour of there being one pole with two foci of intensity in each hemisphere, and reasons will be given further on which tend to throw doubt on there being any revolution of these two magnetic poles round their adjacent terrestrial poles.

Subsequently to Hansteen's charts there appeared those of the Declination by Yeates, Duperrey, and by Barlow in 1836. These were useful to navigation but helped very little towards the solution of the problem of the ever variable distribution of the earth's magnetism.

Besides this by the year 1835 the iron-built ship had appeared on the ocean and a correct knowledge of the

three magnetic elements became a necessity in solving the problems which the magnetism of different iron ships presented.

With Gauss's invention of the absolute horizontal force magnetometer in 1833, many hitherto unknown movements of the magnetic needle of the highest interest were discovered, which with the coarser instruments previously in use lay concealed. This discovery gave the desired impetus to the scientific men of that epoch, and the period included in the years 1835-45 was "a time of unparalleled activity in the extension of systematic and accurate magnetical observations over the earth's surface".

Whilst most of the continental nations joined in this movement, the principal share in the work was divided between Germany, Russia, and England in Europe, and the United States in America. But before the splendid series of simultaneous observations made on the continent, and four British colonial observatories were organised, Gauss in 1839 published his general theory of Terrestrial Magnetism coupled with a series of charts of the three magnetic elements for the whole world, based upon observations made at ninety-two selected stations distributed over the earth's surface; and it may be remarked that Gauss's charts not only gave results in fair accordance with observation in explored regions, but also as afterwards proved in Antarctic latitudes hitherto unvisited by man.

The proof came in the years 1839-43, when Ross's Antarctic voyage of exploration was carried out in the interests of terrestrial magnetism. Besides the importance of a knowledge of the general distribution of the magnetic elements in those regions, one great aim of this expedition was to reach the south magnetic pole. This was found to be impossible, but sufficient data were collected to give its approximate position. Whilst this Antarctic magnetic survey was being completed, that of British North America was also undertaken with the result of the determination of the locality of one of the foci of greatest intensity in the northern hemisphere.

The results of these surveys formed, as will be well

remembered, a valuable series of "contributions" to terrestrial magnetism by Sabine, and, coupled with every available observation between the years 1818 to 1876, formed the materials for the series of charts entitled "The Magnetic Survey of the Globe" for the epoch 1842·5. Each map gave normal lines of equal values of the Declination, Inclination and Intensity. Although it may be said that from the Arctic circle to the Antarctic, the direction of the lines was efficiently given by observation, the lines within those circles were largely taken from Gauss's computed lines modified to agree with observation.

Another difficulty in compiling these charts of Sabine's with accuracy lay in the uncertain knowledge of the secular change then available, and which had to be applied to observations made so far apart in time.

Sabine's charts are doubtless the best we have for the epoch 1842·5, but in the light of the requirements of modern science they leave much to be desired as regards the Antarctic regions. The observations south of 60° S. were made entirely on board ships, where the errors of the compass sometimes exceeded 50° due to the horizontal forces in the ship, thus rendering accurate observations of the Declination very uncertain and correction of the observed Inclination very difficult; besides which there are no records of the ship's disturbing force in the vertical direction.

Naval requirements, however, did not permit of any delay in publishing magnetic charts affecting navigation, for in 1846 the Hydrographer of the Admiralty requested Sabine to provide charts of the Declination for the Atlantic Oceans from 60° N. to 60° S. These were largely used until Evans's chart of the Declination for the whole navigable world was issued in 1858.

The excellent work of Flinders already referred to, of ascertaining from his knowledge of terrestrial magnetism the chief cause of the deviation of the compass in wood-built ships, and providing a corrector for those deviations, had to be followed up on a much larger scale and with more exact methods in the iron-built ship, which, in that

period of activity in terrestrial magnetic science—1835-45—was rapidly increasing in numbers on the ocean.

Thus in 1835 observations were made on board iron ships showing that they acted as a magnet on their compasses, but there was nothing to show in the results what the causes of this condition of the iron ship were, until Poisson in 1838 published his celebrated "Memoir on the deviations of the compass produced by the iron in a ship". This was a rigorous mathematical investigation of the subject, showing that the deviations of the compass were due to induction in the ship by the magnetic force of the earth.

If the iron ship had simply been built for service in one locality, a limited knowledge of terrestrial magnetism would have sufficed to elucidate the causes of her magnetic condition; but she was destined to traverse every navigable sea over large changes of magnetic latitude, hence the necessity for an accurate knowledge of the distribution of magnetism over the great parent magnet, in order to determine the magnetic condition of her comparatively minute offspring the magnetised iron ship; and this at all times and in all places in the interests of navigation. Observations of the terrestrial magnetic elements were therefore an absolute necessity if iron-built ships were to be substituted for those of wood.

The ability to predict the deviation of the compass on change of latitude did not, however, satisfy Airy, for after a remarkable mathematical investigation of iron ship's magnetism of a less rigorous character than Poisson's, but sufficiently accurate for his purpose, he in 1839 proposed his methods of annulling the deviation of a ship's compass by means of magnets and soft iron, so arranged as to produce equal and opposite magnetic effects to that of the ship. Provided with Airy's admirable and simple directions this method of correction was comparatively easy in one latitude, but experience at sea, especially in voyages to the Cape of Good Hope, showed that every iron ship required a different application of Airy's correctors.

To discriminate between the amount that was to be

corrected by permanent magnets, by horizontal soft iron, and by vertical soft iron, an accurate knowledge of the magnetic elements Dip and Intensity obtained from observations on land and at sea was essential.

Before dismissing the subject of the above application of magnetic observations, it may be remarked that we have now heavily armed, protected steel cruisers steaming over all parts of the world with less change of deviation of the compass than the wood-built *Erebus* and *Terror* of Ross's Antarctic expedition, and this remarkable result could not have been achieved if the terrestrial magnetic observer had not done his work.

Moreover, if magnetic observations are not continued the secular change of the magnetic elements will soon commence to mar the precision with which our rapidly moving ships traverse the globe.

The voyage of the *Challenger* in 1872-76 contributed the most valuable series of observations of the magnetic elements in modern times, when the large areas of the principal oceans traversed by that vessel during three and a half years are taken into consideration. These observations, combined with those taken from every available source, both British and foreign, between the years 1865-87, formed the materials from which the magnetic charts of 1880 were compiled (see vol. ii., *Physics and Chemistry*, part vi., *Voyage of H.M.S. "Challenger"*).

The *Challenger* only crossed the Antarctic circle at one point in longitude 78° E., and, therefore, although we know large secular changes to be going on south of 40° S. we have no measure of the amount, nor anything like an accurate knowledge of distribution of the earth's magnetism in those regions. This points to the necessity for a new Antarctic expedition.

In the year 1888 the late Professor J. C. Adams was provided with a complete set of magnetic charts for the two epochs 1842·5 and 1880 previously mentioned, and as it was known he had directed his profound mathematical ability to the analysis of the results contained in them, it was hoped that some new and important light might be

thrown upon the bare facts presented. His lamented death occurred without his publishing any results.

If, however, reference be made to the report on the magnetical results of the *Challenger*, a discussion of the secular change is contributed founded in a great measure on a comparison of those charts. The outcome of this discussion is to throw considerable doubt upon the theory that the motion of the magnetic poles round the terrestrial is the cause of secular change; in fact, that the magnetic poles remain fast, and we must look elsewhere for the cause whatever it may be.

Magnetic observations have so far been considered in their all-important bearing as necessary to safe navigation in wood-built ships, and in a far higher sense as indispensable to that of the iron- or steel-built ships which now cover the ocean; the magnetic charts hitherto generally required for these purposes being those on which normal lines of equal values have been given, but something more is now needed.

Observation in comparatively recent years has shown that not only are there large "regional" magnetic disturbances extending over large areas of land, but that in moderate depths of water where the largest ship can navigate freely, the land below is also found to have considerable areas of local magnetic disturbance which, if not allowed for, may in thick or foggy weather lead ships into danger by seriously disturbing their compasses.

The United States have done excellent work in producing charts of iso-magnetic lines, or charts in which the chief local magnetic disturbances are recognised, and the full results of observation recorded. The magnetic surveys of Rücker and Thorpe in the British Isles, of Moureau in France, of Rijckevorsel in Holland and elsewhere, have thrown considerable light on the magnetic conditions of those countries, but there remain whole continents to be covered by the observer.

The direction of the iso-magnetics too from the deep sea to the dry land of the coasts is an extension of the subject, which the observer has hardly touched as yet, but

one affecting the safety of navigation, as well as the question that has been raised, whether the water areas of the globe are as a whole more, or less magnetised than the land areas.

To possess charts of iso-magnetic lines for even a few countries is an evidence of considerable advance in the knowledge of terrestrial magnetism, for if reference be made to Sabine's lines of intensity in his contribution on the magnetic survey of North-West America it will be found that he rejected certain observations he considered abnormal and defective, which Lefroy the observer considered to be his best and naturally retained in his map; the result being a considerable difference in the form of the curves adopted by the two magneticians, Sabine giving normal curves, Lefroy iso-magnetics.

Respecting the local disturbances of the needle which have been so clearly proved, the question naturally arises, whence the cause of these disturbances? It is now believed by many, if not finally accepted, that Rücker and Thorpe have answered the question by the results of their laborious survey of the British Isles, coupled with Rücker's elegant investigations as to the permeability of specimens of the rocks taken from the localities in which magnetic disturbances were found. Their answer is to the effect that these disturbances, which have been found to extend over a region 230 miles long by about 110 miles broad, are due to induction by the earth's magnetism in rocks of different permeability, either present as in the basalts on the surface or concealed by superficial deposits.

These results are distinct from the extraordinary disturbances of the needle when in the immediate vicinity of permanently magnetised rocks, and when the radius of disturbance may be only as many feet as the extent of the regional disturbance is in miles.

The points of interest in the question of regional magnetic disturbance are not confined to the magnetician, for the geologist cannot afford to neglect the valuable information the magnetic needle affords. Thus although Rücker and Thorpe have since made a second and more elaborate survey of the British Isles, their remark of 1890

that "the kingdom can be divided into magnetic districts in which the relations between the direction of the disturbing forces and the main geological characteristics are so suggestive as to be worthy of careful statement and further investigation," not only holds good, but has received confirmation.

The mining engineer is deeply interested in a knowledge of the Declination. Charts of normal lines are of great use to him whether above or below the earth's surface, but especially below when he has no other guide. To such an one a knowledge of regional magnetic disturbance as deduced from surface observations is most important, as it tells him that he is in the neighbourhood of magnetic rocks, the disturbing effect of which on his compass needle may be far greater in the depths of his mine and turning it into a treacherous guide.

We have now considered magnetic observations in a measure from the point of view of the immediate practical results which their scientific treatment produces, but who will say in this great maritime nation that the work of magnetic observers, even if solely to make navigation possible, is not worthy of the fullest consideration?

There is besides a vast field of inquiry for the observer of terrestrial magnetism in unravelling the secrets of the earth considered as a magnet, and the ceaseless change of its magnetic condition which the needle tells us of, for which no immediate practical result can be foreseen, yet is worthy of the attention of the ablest physicists and most advanced mathematicians.

Inquiry into the causes of the secular change is one requiring the fullest attention, but observation has not yet done sufficient work. It certainly has done much in certain countries, and for a large portion of the world as regards secular change in the past, and data obtained for predicting future changes for a few years, but only one expedition has examined the Antarctic regions magnetically, and it is doubtful if any substantial progress will be made until a second expedition is made thither, one profiting by the experience of its precursor, and equipped with possibilities for work hardly hoped for by Ross.

It may be remarked in passing that a remarkable alteration in the amount of the secular change has been noticed in the Declination and Inclination at the following observatories: Bombay, Batavia, and Hong Kong about the period of the eruption of Krakatoa in 1883. This may be only a coincidence, but may it not also point to the possibility that the changes below the surface of the earth which culminated in that mighty explosion, and may still be at work, have had, and continue to have, magnetic effects which are recorded by the needles at those observatories?

Critical investigations have for many years been directed to the elucidation of the causes of the observed diurnal variations of terrestrial magnetism. This work was long seriously retarded by the various methods adopted at different observatories for recording their results, obliging those who entered upon a comparison of such results to go through a tedious conversion of them into a common method. It may be said that the first large departure from this objectionable practice occurred when the International Polar Inquiry of 1882-83 was undertaken by the various expeditions.

This was an important step in the right direction, but there remains much to be done, as shown by the ten reports of the British Association Committee on "the best means of comparing and reducing magnetic observations". Their last report consists of an able and suggestive paper by Dr. Chree, being the analysis of the results of five years' observations on "quiet days" at Kew, and is well worthy of attention as indicative of the present state of our knowledge as regards the diurnal variation of the three magnetic elements.

Such investigations only encourage one in the hope that the much required observations in southern latitudes may be undertaken. The observatories at the Cape and Melbourne could do invaluable work if it were carried out on the lines of Kew, for example.

Lastly, what more is there to be said about magnetic observations and their bearings? We do not know why the earth is a magnet, the cause of the secular change of its

magnetism, why it is subject to solar diurnal, lunar diurnal, sidereal diurnal and the other variations and disturbances, nor the cause of magnetic storms, although we can observe connections between them, earth currents, and auroræ. Whether the causes of all these exist below the surface of, or are external to, the earth, or are a combination of the two, has still to be learnt, and it seems hardly too much to hope that the restless needle will sooner or later be the means of opening up sources of knowledge invaluable to cosmical science, as well as to those only concerned with the planet upon which they dwell.

When the causes of the secular change are understood there will be no difficulty in providing the navigator with magnetic charts for years in advance, much as the tides can now be tabulated for his use. In the latter case observation has done its work for several frequented ports, in the former case a vast amount remains to be done, and the word that goes forth is still, as Lord Kelvin thrice said on a kindred subject connected with ships' magnetism, "Observe".

ETTRICK W. CREAK.

# THE PRESENT POSITION OF THE CELL-THEORY.

## PART I.

A FEW years ago a discussion of the cell-theory would have seemed superfluous. To-day, partly because of criticisms which have been directed against the theory, partly because of the great increase of our knowledge respecting cell-structure, the advantage and even the necessity of such a discussion will be admitted by everybody who has read and reflected on the subject. In what follows, I propose to examine the cell-theory in the light of recent criticisms and researches. I set out with the intention of avoiding anything in the shape of polemical writing, but I fear that I have in places fallen away considerably from the course which I had proposed. In a much disputed subject controversy is inevitable, a circumstance which need not be regretted, for controversy is the whetstone of argument, and obliges those who engage in it to be doubly careful both of their facts and of the language in which they express them. My antagonists will, I hope, give me the credit of the desire to deal fairly with their arguments and criticisms, and will acquit me of unnecessary bitterness. It has been my object to elucidate the subject in hand rather than to try to gain a dialectical advantage.

It is advisable, before entering on the examination, to have a clear conception of what the cell-theory really is. This is the more necessary because one of its most recent critics, Mr. Adam Sedgwick, has complained than nobody will define the theory in an exact manner; it is, he says, a kind of phantom which takes different forms in different men's eyes. I have shown in another place that this statement is hardly fair, because there are some authors whose researches on cytology entitle them to speak with authority who have recently defined the cell-theory in a very precise manner, though it may be conceded that there are biologists

whose views are not so exact, and who habitually commit themselves to statements which on careful examination may prove to be altogether untenable.

It was pointed out some time since by Whitman,[1] and I have since emphasised the fact,[2] that in his broad generalisations Schwann defined the cell-theory in a very exact manner, and that the words originally used by him are perfectly applicable to the cell-theory as it has been held up to the present time. In saying this, I do not forget that Schwann held some very erroneous views as to the nature and structure of cells, which he regarded as vesicles, filled with fluid, which made their appearance in a structureless matrix, named for this reason, a cytoblastema. But Schwann's work consisted of two parts, a statement of observations, which have proved to be entirely erroneous, and a theory of organisation, which has been very fruitful of results. He was careful to say that his theory was only a provisional explanation which suited the facts as nearly as possible, and it is a great merit of the theory that it afforded such an insight into organisation that the essential part of it did not cease to be serviceable long after the "facts" on which it was founded were shown to be, for the most part, false. We need not therefore concern ourselves with the fact that Schwann's conceptions of the origin and structure of cells were false, but we may examine his theory and see how much of it we may hold to, and how much we must reject at the present day.[3]

Schwann was a very cautious writer, and the quotations which are given below will dispose effectually of the state-

[1] C. O. Whitman, "On the Inadequacy of the Cell-theory of Development," *Journal of Morphology*, viii., p. 639, 1893.

[2] G. C. Bourne, "A Criticism of the Cell-theory," *Quart. Jour. Microscopical Science*, xxxviii., p. 137, 1895.

[3] A large part of Schwann's theory of cells, *viz.*, that part of it which compared cell-formation to the process of crystallisation, was soon shown to be untenable. But as this part was based on his erroneous views on the structure and origin of cells, I have passed it over, since the falsity of his views on this subject involved the falsity of as much of his theory as was founded on them.

ment which stands in the first paragraph of Whitman's work, that he believed that in cell-formation *lies the whole secret of organic development.* There are, says Schwann, two possible theories on the subject of organic development: (1) The organism theory, namely, that there is an inherent power modelling the body in accordance with a predominant idea. (2) The physical theory, namely, that the fundamental powers of organised bodies agree essentially with those of inorganic nature. Rejecting the former of these two theories as being outside the domain of physical science, Schwann went on to write:[1] "We set out with the supposition that an organised body is not produced by a fundamental power which is guided in its operation by a definite idea, but is developed according to the blind laws of necessity by powers which, like those of inorganic nature, are established by the very existence of matter. As the elementary materials of organic nature are not different from those of the inorganic kingdom, the source of the organic phenomena can only reside in another combination of these materials, whether it be in a peculiar mode of union of the elementary atoms to form atoms of the second order, or in the arrangement of these conglomerate molecules when forming either the separate morphological elementary parts of organisms, or the entire organism. We have here to do with the latter question solely, whether the cause of organic phenomena lies in the whole organism or in its separate elementary parts. If this question can be answered a further inquiry still remains as to whether the organism or its separate elementary parts possess this power through the peculiar mode of combination of the conglomerate molecules or through the mode in which the elementary atoms are united into conglomerate molecules."

Is it not perfectly clear from this that Schwann fully recognised that there was a further question underlying

[1] Th. Schwann, *Microscopical Researches into the Accordance in the Structure and Growth of Animals and Plants.* Translated by Henry Smith. London: Printed for the Sydenham Society, 1847.

the cell-theory, and do not the words which he used with regard to the union of elementary atoms to form atoms of the second order show a prescience of the assumptions which would have to be made to explain the powers manifested by cells? Because he confined himself to the one question, it is not fair to say that Schwann had not a clear appreciation of the importance of the other. I may relate, in this connection, an anecdote which will finally clear Schwann's reputation from the reproach fastened upon it. Professor Lankester tells me that about the time when a sort of jubilee was held in Schwann's honour at Liége in 1878, he was introduced to him, and ventured to ask in the course of conversation how it was that after the publication of his famous essay he had so long been silent. Schwann answered that he had not been idle, but that ever since he had been unsuccessfully occupied in trying to find out the secret of the constitution of the cell.

To return to the question propounded by Schwann, does the cause of organic phenomena lie in the organism or in its separate elementary parts, the cells? He answers very decidedly, in the separate elementary parts, and gives the following reasons for his answer: "All organised bodies are composed of essentially similar parts, namely, of cells; these cells are formed and grow in accordance with essentially similar laws, and therefore these processes must in every instance be produced by the same powers. Now if we find that some of these elementary parts not differing from the others are capable of separating themselves from the organism and pursuing an independent growth, we may thence conclude that each of the other elementary parts—each cell—is already possessed of the power to take up fresh molecules and grow, and that therefore each elementary part possesses a power of its own, an independent life, by means of which it would be enabled to develop itself independently if the relations which it bore to external parts were but similar to those in which it stands in the organism. The ova of animals afford us examples of such independent cells apart from the organism."

A little further on he says: "In inferior plants any given

cell may be separated from the plant and can grow alone. So that here are whole plants consisting of cells which can be positively proved to have independent vitality. Now as all cells grow according to the same laws, and consequently the cause of growth cannot in one case lie in the cell, and in another in the whole organism, and since it may be further proved that some cells, which do not differ from the rest in their mode of growth, are developed independently, we must ascribe to all cells an independent vitality, that is such combinations of molecules as occur in any single cell are capable of setting free the power by which it is enabled to take up fresh molecules. The cause of nutrition and growth resides not in the organism itself but in its separate elementary parts. . . . The manifestation of the power which resides in the cell depends upon conditions to which it is subject only when in connection with the whole organism."

The whole theory is very succinctly summed up in the following passage: "The elementary parts of all tissues are formed of cells in an analogous though very diversified manner, so that it may be asserted that there is one universal principle of development for the elementary parts of organisms, however different, and that this principle is the formation of cells".

No doubt objection may be taken to-day to the universality of this statement, but if we modify the last part of it and read "that the most general principle of development for organisms, however different, is the formation of cells," we shall have very nearly expressed the truth, as we know it at the present day.

I have found it necessary to quote Schwann's work at considerable length, and to repeat more emphatically what I stated in my previous essay on Epigenesis and Evolution.[1] Dr. Whitman,[2] in a reply which deals partly with my

[1] G. C. Bourne, "Epigenesis and Evolution," "SCIENCE PROGRESS," vol. i., 1894.

[2] C. O. Whitman, *Evolution and Epigenesis*. Boston: Ginn & Co., 1895.

criticisms, and partly with the much more weighty arguments brought forward at the same time by Dr. Oscar Hertwig, says that my criticisms, in so far as they are directed against the inadequacy of the cell-theory of development, are largely the result of misunderstanding; this may in part be true, but I cannot have misunderstood the simple meaning of his first paragraph, and I wish to insist on the fact that the cell-theory, as it was promulgated by Schwann, did *not* regard cell-formation as the whole secret of organic development, and that the cell was not, in the mind of the author of the cell-theory, the alpha and omega of both morphological and physiological research in the animal kingdom. If this is clearly understood at the outset, it will help to remove much possible misunderstanding.

But, as Mr. Sedgwick has rightly said, we have to deal not only with what its authors thought, but with the cell-theory as it is understood and taught at the present day. I have already pointed out [1] that the most recent definition of the cell-theory is, to all intents and purposes, identical with the broader generalisations of Schwann. Dr. Oscar Hertwig writes: [2] "Animals and plants, so dissimilar in their outward appearances, are similar in the essentials of their anatomical structure, since both are composed of similar elementary parts which for the most part are only recognisable by the microscope. . . . Since the cells, into which the anatomist resolves vegetable and animal organisms, are the bearers of the vital processes, they are, as Virchow has expressed himself, the vital units. Viewed from this standpoint the whole life process of a composite organism appears to be nothing else than the extremely complicated result of the individual life processes of its numerous and variously functional cells." This is simply a restatement in other words of two of the fundamental principles of Schwann, namely (1) that the elementary parts of all tissues are formed

[1] G. C. Bourne, "A Criticism of the Cell-theory," *Quart. Jour. Micr. Science*, vol. xxxviii., p. 137, 1895.

[2] O. Hertwig, *Die Zelle und die Gewebe*. Berlin: R. Friedlander und Sohn, 1893.

of cells; (2) that the cause of nutrition and growth resides not in the organism but in its separate elementary parts.

The attacks which have recently been directed against the cell-theory may be resolved into contradictions of these two fundamental propositions. On the one hand there is the considerable number of cytologists, whose opinions may be taken to be summed up in Whitman's essay on the inadequacy of the cellular theory, who deny the second proposition, and in so doing implicitly deny the truth of the first. They would say that the cause of nutrition and growth does not reside in the cells considered as elementary parts, but in parts still more elementary, the ultimate vital units of which the cells themselves are composed. On the other hand Mr. Adam Sedgwick denies the first proposition *in toto*. He states boldly that the elementary parts of tissues are not formed of cells, but of a continuous mass of vacuolated protoplasm containing nuclei.[1] These objections, though they are raised from different standpoints, are not irreconcilable, but it will be convenient to deal with them separately. First let us consider the objections to the cell as an ultimate vital unit.

These objections are of long standing. They were first brought forward by Brücke[2] in 1861;[3] not long afterwards

[1] Since this was written Mr. Sedgwick has published a further account of his views, which makes it necessary to modify this statement. See *infra*.

[2] Ernst Brücke, "Die Elementarorganismen," *Sitzungsberichte der K. Akademie der Wissenschaften*, Wien, bd., xliii., p. 381, 1861.

[3] Delage points out that the merit of regarding protoplasm as an organised substance belongs to Dujardin, and not to Brücke. At the same time he points out the essential difference between Brücke's concept of organisation and that of Dujardin, greatly to the advantage of the latter: "La difference entre Dujardin et Brücke est très simple. Le premier a deviné l'existence de *structures* que le microscope démontre aujourd'hui; tandis qu'en introduisant dans la conception de protoplasma cette notion acceptée avec enthousiasme, d'organismes très compliqués et invisibles, Brücke a ouvert la porte aux nombreuses théories speculatives qui cherchent á imaginer la structure de ces organismes pour expliquer par elles les phénomenes de la vie." Delage adopts the expression organisation, saying: "Le protoplasme n'est pas simplement, comme on l'a cru longtemps, une substance chimique *organique*, mais il est *organisé*, c'est-â-dire possede une

Herbert Spencer followed with his theory of physiological units. Darwin's theory of pangenesis expressed the same idea, and more recently Nageli, De Vries, Wiesner, Weismann and others have entered the same or at least similar objections to the cell-theory. Even Oscar Hertwig, although he appears in the sentence above quoted to give his adherence to the view that the cell is a vital unit, abandons this concept, for all practical purposes, in the latter part of his book; for he says, in a most unmistakable manner, that the cell is an organism composed of ultimate units which he calls idiosomes.

Each author whose name I have quoted has a somewhat different account to give of the ultimate constitution of the cell. But the points on which they disagree are of subordinate importance; they are all agreed on the main issue, that the vital activities manifested by the cell are not to be explained by the visible constitution and structure of the cell itself, nor by the mere chemical elements of which the protoplasm of the cell is composed. Each of them avers that the cell is *organised*, which means that it is made up of countless organic units of a lower order, differing among themselves, and arranged in groups and sub-groups within the cell in a manner analogous to that in which the cells themselves are arranged in a composite organism. Since there is so general an agreement in fundamental principle, I am spared the necessity of examining each separate theory of ultimate vital units in detail; should anybody wish for a condensed account of the various theories he will find it in Weismann's introduction to his work on the Germ Plasm.[1]

structure d'un ordre plus élevé que la structure atomique des molécules chimiques des composés organiques non vivants". No fault can be found with this definition, but would it not be better to adopt some other term to express this extra complexity of *structure* rather than "organisation," which is inseparably connected with our ideas of the composition of the bodies of higher animals and plants? For Brücke the organisation of protoplasm was the same in kind as the organisation of higher animals: for Dujardin it was something different, and had best be expressed by a different term. Delage puts the word structure in italics.

[1] Still better in Delage's book, referred to further on.

The point for present consideration is this: Is it necessary for the explanation of vital phenomena to assume the existence of ultimate vital particles, so minute as to be invisible with the best microscopical powers which we possess, each of which is to be considered as being *in posse* an independent organism capable of displaying some of the most characteristic of vital phenomena, *viz.*, assimilation, growth, metabolism, reproduction by division?

As it will be necessary to refer frequently to these assumed vital units, I must call them by some name, and I shall use Weismann's term biophor, without meaning to exclude the conceptions of other authors, the pangenes of De Vries, the plasomes of Wiesner and so forth. I use the term biophor in the sense of *Lebensträger*, the bearer of the vital properties, without necessarily implying that it possesses all the particular properties assigned by Weismann to his biophors.

Whatever the point from which the different authors have started, they all postulate the existence of minute biophors on the grounds that the vital phenomena exhibited by cells, say by an Amœba, or by the ovum of a Metazoon, imply the existence of an organisation adequate to the production of the observed processes. The life processes are too various and too complicated in their kind to be explained by the visible constitution of protoplasm, even if it be allowed, as it generally is allowed, that protoplasm is not a chemical compound of fixed molecular composition but a mixture of many chemical substances, each having a molecular composition of some considerable complexity. I have already shown that Schwann himself was possessed of such an idea, which he expressed sufficiently clearly when he referred to "a peculiar mode of union of the atoms to form atoms of the second order," but he did not attempt to follow out the idea, confining himself to the inquiry into "the arrangement of these conglomerate molecules when forming either the separate elementary parts of organisms or the entire organism". The term conglomerate molecule is in fact synonymous with the term biophor

in the sense in which I am using it, for the biophor or ultimate vital unit is held to be an aggregate of chemical molecules; the constitution attributed to it is that it is made up of many different kinds of molecules, just as a molecule may be composed of several different kinds of atoms. I shall have to refer again to the difficulties which still remain if we accept the hypothesis that a group of different molecules is able to exhibit the vital functions which are necessarily attributed to a biophor. Before proceeding to criticism I must try to give as fairly as I can the grounds for believing in the existence of biophors. To put the matter as briefly as possible, the theories of ultimate vital units are the result of attempts to make a mental analysis of living substance. Chemical analysis is impossible, for in the process the living substance is destroyed as such and becomes dead substance, possessed of different and much less important properties. One fact of great importance, however, is learnt from chemical analysis, and it was appreciated by Schwann, namely, that, to use his original words, "the elementary materials of organic nature are not different from those of the inorganic kingdom"; hence it has been inferred, with all reason, that the powers of organic nature are essentially the same as those of inorganic nature, and are established by the very existence of matter. It is only necessary to mention this because there has recently been a tendency in some quarters to call in the assistance of some mysterious "vital force"; a tendency begotten no doubt by the apparent futility of all attempts to find an explanation on mechanical and chemical principles of the fundamental powers of organic nature, assimilation and metabolism. This, however, need not detain us; we have to consider the process of reasoning which, in default of assistance from chemical analysis, has led so many distinguished observers and thinkers to analyse the cell into other components, and those again into others of a lower grade, until the biophor, the smallest particle of possible life, is reached.

The weightiest reason which I have been able to dis-

cover is given by von Sachs.[1] According to this author, whose views are in agreement with those of Nägeli on this subject, it is necessary for the explanation of certain phenomena exhibited by organic substances that we should assume the existence of combinations of molecules which form very large numbers of small particles or micellæ as Nägeli calls them. One of the most important of these phenomena is the imbibition of water. Dry organic substances, such as gelatine, when placed in water, imbibe it and increase in volume to a very considerable extent. The increase in volume produced by the swelling up in water is almost equal to the volume of water which has been absorbed. The imbibition of water in such a case is something very different from the imbibition of water by a porous inorganic body, such as gypsum, unglazed porcelain, etc. The latter substances are full of small visible and invisible cavities or pores, which in the dry state contain air. The water passes into these cavities or pores according to the laws of capillarity, and in so doing displaces the air, which is forcibly expelled and can be collected and measured; there is no pushing asunder of solid parts, as is shown by the fact that the porous body is not perceptibly enlarged by the water which has penetrated into it. But the water penetrating into gelatine expels no air, it does not enter by capillarity into spaces previously existent, but forces its way between the particles of the dry substance, pushing these asunder, and so causing the considerable increase in volume. The particles thus pushed asunder are the micellæ, and although they are pushed further apart from one another, they do not completely lose their connection. Each micella may be regarded as being surrounded by an envelope of water when in the moist state; in the dry state the micellæ composing the substance are in mutual contact. This familiar phenomenon of the swelling of organic substances by the imbibition of water is contrasted by von Sachs with the process of solution of a salt. In the latter case the water

[1] J. von Sachs, *Lectures on the Physiology of Plants*, translated by H. Marshall Ward. Oxford, Clarendon Press, pp. 205 and *sq.*, 1887.

seizes on the molecules of a crystal and takes them in between its own molecules ; in the former case the dry organic body seizes on the molecules of water and forces them between its own. These reasons are held by von Sachs and Nägeli to be among the weightiest for regarding protoplasm as an "organised" body, in the sense of being made up of micellæ, and not as being a structureless slime or fluid.

No doubt they are weighty reasons for regarding organic substances such as gelatine, starch grains, cell walls, etc., as being composed of combinations of polyatomic molecules into groups of a higher order, and there is no objection to giving these groups a name, such as micellæ. But the admission that such groups exist does not really bring us much nearer to an explanation of the phenomena of life. Von Sachs himself points out that even in the region of pure chemistry it is necessary to assume that polyatomic molecules are grouped into closer molecular unions, thus giving rise to chemical properties which did not belong to the individual molecules.

Gelatine, starch grains and cellulose are not living but dead substances, and the fact that the behaviour of dead organic substance finds an explanation on a theory of micellar structure is but a very small step towards the explanation of the very different behaviour of living substance. The micellæ may exist in the organic substances in question, but they are not to be confounded with biophors; the very fact that the properties of dead substances may be attributed to their existence shows that they cannot be considered as bearers of vital properties.

In point of fact the living substance, which we generalise under the name of protoplasm, behaves quite differently in respect of the imbibition of water to the dead substances which are derived from it. An amœba or an infusorian, living in the water, does not imbibe it as a mass of gelatine would. But when it dies in the same water it immediately begins to swell up, and eventually bursts and disintegrates. So that we see that with respect to this very property which is held to be a reason for assuming a micellar structure of

protoplasm, the actual living substance does not exhibit the property, whilst the same substance when dead does. Clearly then, the admission that protoplasm has a micellar structure, that is, that it is composed of minute and invisible particles consisting of groups of polyatomic molecules, does not involve the admission that there are ultimate vital units, biophors, which reside in the cell-like organisms within the cell-organism. This distinction indeed has already been made and dwelt upon at some length by Weismann (*op. cit.*, pp. 41 and 42).

It follows then that whilst we may freely admit that protoplasm, and also various inert organic substances, are composed of micellæ, and are therefore "organised" in the sense spoken of by von Sachs, we have still to consider the evidence for the existence of biophors. At the outset of this inquiry we meet with a difficulty in that the existence of biophors is assumed by most authors as a means of explaining the phenomena of heredity, and this opens up a wide range of questions into which it is not the purpose of this essay to enter. But it has well been pointed out by Wiesner that if minute vital elements occur at all, those same units which make life possible, and control assimilation and growth, must also be the agents in bringing about the phenomena of heredity. This view, which commends itself to everybody, implies that the biophors have only secondarily acquired historic qualities, and that they are primarily concerned in the production of the fundamental processes of life. We may therefore dismiss for the present purpose the complications introduced by heredity and confine our inquiry to the functions of biophors as bearers of the essential vital qualities.

It is urged in favour of a theory of biophors that life must be connected with a material unit of some sort (Weismann); that function presupposes structure (Whitman), and that the structure necessary for the exhibition of such complicated functions as those of living protoplasm cannot be of such a simple molecular kind even as the micellar structure postulated by von Sachs and Nägeli, but must consist of a definite arrangement, an architecture or organ-

isation of separate living particles, the aggregate functions of which produce the vital phenomena. It is further urged in favour of this view of organisation, that in almost all cells we are able to recognise structures under the microscope each of which behaves in respect of growth and multiplication in a manner analogous to that in which the cell behaves. Not only the nucleus but also the chromatin bodies, the microsomata of which these are composed, the centrosomes, the green chromatophores of plant cells, may be observed to increase in size, *i.e.*, to grow and to multiply by division, and it is held that this is proof that the ultimate particles composing these bodies must assimilate, grow and divide in a manner similar to that in which cells assimilate, grow and divide.

This view, whilst receiving a considerable measure of support from other sources, has been most energetically supported by Wiesner,[1] whose extensive work on the subject has received the weighty approval of Weismann. Wiesner refers in detail to the various structures in the form of granules or corpuscles which may be observed in animal and vegetable protoplasm, and he attributes to one and all of them the powers of assimilation and multiplication by division. Nor does he confine himself to the living substance generally recognised under the name of protoplasm. He labours at great length to prove that the cell wall, so often considered as an inert non-living product of living protoplasm, is not in fact dead, but contains a living substance distinguishable under the name dermatoplasm, and ultimately composed of structural elements of the same fundamental nature as that of the cytoplasm. These ultimate particles are the *plasomes*, which form the central point of his theory of the constitution of living matter. Further than this he accepts in full the theory of De Vries with regard to vacuoles, and considers them to be just as much independent organisms as the chromosomes, the centrosomes, the chlorophyll bodies and other things. This theory of vacuoles, which

[1] J. Wiesner, *Die Elementar structur und das Wachsthum der Lebendem Substanz.* Wien: Alfred Hölder, 1892.

assumes that they are products of minute bodies called tonoplasts, is of itself improbable, and is contrary to the teaching of observations which may readily be made on the constitution and behaviour of vacuoles in living protoplasm. It has been shown by Bütschli[1] that the contractile and other vacuoles of Protozoa continually make their appearance without owing their origin to the division of previously existing vacuoles. It is not possible to go into details here, but the reader will find a full discussion of this question in Bütschli's work (p. 230) as also a *résumé* of the various theories which have from time to time been put forward on the subject of the granular theory of protoplasm. But even if peculiar views on the nature of vacuoles be laid aside, the gist of Wiesner's arguments is not materially weakened. None of the structures which are observable in protoplasm are observed to originate neogenetically: they are all, he says, derived directly by division from pre-existing structures of similar character. In short, he fully accepts the aphorism put forward somewhat earlier by Altmann: "Omne granulum e granulo". Wiesner does not definitely say that the various particles observable in protoplasm are to be severally identified with the ultimate vital units, his plasomes. Some of them may be individual plasomes, but the majority of them are, he thinks, aggregates of plasomes, units of a higher order which in turn are combined to form the still higher unit the cell. Thus he presents a scheme of organisation which, instead of taking the cell as the lowest structural unit, goes several grades lower; instead of the old conception of

organ—tissue—cell,

he represents the scheme of organisation as being

organ—tissue—cell—granules—plasomes.

A detailed criticism of Wiesner's views would occupy a much larger space than I have at my disposal, and such a criticism is unnecessary, since all that need be said has

[1] O. Bütschli, *Investigations on Microscopic Foams and Protoplasm*. Authorised translation by E. A. Minchin. London: A. and C. Black, 1894.

already been set forth by other authors in their criticisms of similar theories, particularly by Bütschli (*loc. cit.*, p. 195) and O. Hertwig, both of whom occupy themselves with Altmann's views, which are to all intents and purposes identical. Only a few of the most important points need be touched upon here.

It is certainly a remarkable fact, and confirmed by abundant experience, that many of the constituent parts of cells are produced by divisions which recall the divisions of the cell itself. The nucleus is the most important and the most familiar constituent of the cell: it is within the experience of every biologist that nuclei are never observed to originate neogenetically, but always by division of a pre-existing nucleus. The chromatin elements of the nucleus may be shown to be composed of minute particles, the so-called chromosomes, and these reproduce themselves by division, and are never observed to originate neogenetically. The same statement holds good for the centrosomes, for chlorophyll corpuscles and for various kinds of chromatophores. It is not to be denied that these facts, which become more and more familiar to the working microscopist, appear to lend a powerful support to the theory of biophors; in a limited sense they may be said to be a proof of the statement that the cell is an organised body. Whether, as Wiesner claims to be the case, there are many other constituents of cells which similarly reproduce their kind by division, and are never observed to originate independently, may for the present be left out of consideration. The evidence that amylum grains and granules of various kinds behave like the centrosomes in this respect, is as yet too slight, and the observations are too conflicting to enable us to come to a judgment without entering into a mass of detail which is not wholly relevant to the question at issue. But there is at least one criticism which is worthy of mention, namely, that of Bütschli, who points out (*loc. cit.*, p. 200) that among the strongly staining granules of protoplasm there are bodies which are not actually constituents of the protoplasm but are symbiotic micro-organisms. The existence of such organisms, which have been called

Bacteroids, has been frequently demonstrated in animal and vegetable cells, and Bütschli points out that granules similar in appearance to bacteroids occur in the Vorticellinæ and may be observed at certain times to be in a state of rapid proliferation.

Just before writing these pages I have been shown preparations exhibiting the numerous bacteria symbiotic in Pelomyxa, and it is very possible that the rapid proliferation of bacteroids has been mistaken by various observers for the reproductive activity of granules forming an integral part of the cell. It is almost certain that the mistake has been made in some cases, and until further investigation has increased our knowledge of the various micro-organisms which are symbiotic or parasitic in cells, it is well to be somewhat sceptical of statements regarding the divisional processes of cell contents. It would seem then that our present knowledge does not justify our regarding all the particles of a cell as originating in a similar manner from the division of pre-existent similar particles, though we must affirm in the most positive manner that some few of the constituents of the cell originate in this way only, and are never produced *de novo*. The question now to be considered is this: must we, because these bodies (the centrosomes, chromosomes, etc.) assimilate, grow and reproduce themselves by division, regard them as independent vital units? A cell exhibits these phenomena and the cell is regarded as an independent unit *in posse*, if not actually *in esse*; must we therefore attribute to all bodies exhibiting the same phenomena the character of independent units? The answer, I believe, is very decidedly no. Cells would never have been regarded as independent units if they had merely been observed to assimilate, grow and divide, whilst retaining their connection with other cells undergoing the same processes. The quotations which I have given from Schwann's work show that the theory that the cell is an independent life unit was not founded on the fact that it assimilates, grows and divides,—Schwann indeed overlooked the phenomena of reproduction by division—but upon the fact that cells are capable of

leading an independent existence. This is so important a part of the cell-theory that I may again quote in his own words Schwann's reasons for calling the cell an elementary unit of life. "Now if we find that some of these elementary parts not differing from the others are capable of separating themselves from the organism and pursuing an independent growth, we may thence conclude that each of the other elementary parts is already possessed of the power to take up fresh molecules and grow, and that therefore each elementary part possesses a power of its own, an independent life."

In the case which we are considering the very faculty which was so powerful a reason for regarding cells as independent units is wanting. Nobody has ever observed a nucleus or a centrosome or even a chromatophore to separate itself from the cell and pursue an independent existence. And not only is there no recorded case of the constituent particles of cells separating themselves spontaneously from the cell, but experiments which have been made with the express purpose of determining whether these particles can live apart from the cell to which they belong have in every case given a negative result. Even the nucleus, highly complicated as it is, and itself composed of smaller particles which may easily be demonstrated, perishes when removed from the cell body. The chromatophores similarly perish, and so no doubt would the centrosomes if it were possible to isolate such very minute particles. Many instances might be cited in proof of this, but it is scarcely necessary to bring forward the details; the reader can obtain them by reference to the works of Nussbaum,[1] A. Gruber and Verworn.[2]

It is of some interest in this connection to contrast the process of reproduction in unicellular and multicellular organisms. In the latter reproduction is effected by the separation of a single unit, a cell, from the aggregate, and

[1] M. Nussbaum, *Biol. Centralblatt*, vol. iv.

[2] Max Verworn, "Die physiologische Bedeutung des Zellkerns," *Pflüger's Archiv*, vol. li., 1892.

the unit so separated has from the time of its separation an independent individuality and eventually reproduces the aggregate. The fact that the union of two cells is commonly necessary for the maintenance of life and the exhibition of the powers of development, need not be urged as an objection to this simple statement of the case, for the facts of parthenogenesis show that the union of two cells is not an essential feature. Now if we adopt Wiesner's scheme, and imagine that organisation does not stop at the cell, but that beyond this there are granules, and beyond these again plasomes, and that the plasomes stand in the same relation to the cell that the cell stands to the multicellular organism; we should expect to find that in the reproduction of monocytial organisms the plasome plays a part analogous to that played by the cell in the reproductive processes of polycytial organisms. But we find nothing of the kind. The monocytial organism reproduces itself in just the same way as the polycytial, by the separation of a cell, complete in all its parts. There is no such thing known, even in cases where a flagellate or a radiolarian breaks up into innumerable particles or spores of extreme minuteness, as the separation of any one individual constituent of a cell possessed of the power of leading an independent existence and in time of reproducing all the other constituents. Every spore, however minute, has its portion of the cytoplasm and its share of nuclear matter. If there are any other constituents, it probably has its share of these also, but one cannot speak with certainty on this point, for positive evidence is wofully deficient. At any rate Wiesner, holding fast to his theory that nothing, not even an amylum or an aleurone grain, is produced neogenetically, is at great pains to prove that in cellular reproduction all the parts of the parent are transferred to the offspring. Assuming that this is so, and remembering that there is abundant evidence that nuclear matter and cytoplasm are always transferred, it is evident that the relation in which the plasomes or biophors, regarded as ultimate vital units, stand to the cell, is not at all the same as the relation in which the cell, regarded as an ultimate unit, stands to the

polycytial organism. Bütschli, in a short but very weighty sentence,[1] brings forward the same argument that I have just used in opposition to Altmann's theory of the part played by granules in the vital processes of protoplasm. In my judgment the argument as far as it goes is a sound one, but I am aware that it does not altogether refute the theory of biophors, but only that part of it which states that as cells are to polycytial aggregates so are biophors to cells. This refutation, however, seems to me to be a considerable gain. For it enables us to apprehend that the structure or constitution of the cell, whatever it may be, is not to be expressed in the same terms as the structure of the higher organisms.

It may be objected that nobody does express the structure of the cell in such terms, but the objection does not hold good. It is true that most authors are more guarded in their expressions than Wiesner, and evade the responsibility of declaring that the biophor is to the cell as the cell is to the polycytial organism, by means of reservations, couched for the most part in terms so ambiguous and even transcendental that the whole issue is involved in an obscurity from which it seems hopeless to try to escape. But these expedients are really of little use. The fact remains that in every case the fundamental idea is the same, that the phenomena exhibited by isolated cells having an independent individual existence are of essentially the same kind as the phenomena exhibited by polycytial organisms and must be explained on the same grounds.

If it be not so, what is the meaning of the argument which was first put forward in definite shape by Brücke, and has been repeated by every author who attacks the question in the same manner that he did, that the com-

[1] "So long as the individual constituents of the cell are not seen to persist when isolated, nor are distinct living phenomena observed in them, it is very dangerous to speak of their life as something which they possess in themselves. They are so far living, as long as the opposite is not proved, in that they are parts of living organism, so that the granula may be living in the same way as the nucleus, even though they no longer betray any sign of life after isolation" (*loc. cit.*, p. 199).

plexity of the phenomena exhibited by individual cells, say by an amœba, is so great, the functions observed are so many and so various in their kind that they can only be explained by the assumption that protoplasm is an organised body? Taking the words of O. Hertwig as a fair expression of current opinions on the life processes of a polycytial organism, "that the aggregate life processes of a composite organism appear to be nothing more than the exceedingly complicated result of the individual life processes of its numerous and variously functional cells," it is evident that to the minds of Brücke and his successors the aggregate life processes of the corpuscle of protoplasm called a cell are nothing more than the highly complicated result of the individual life processes of its numerous and variously functional biophors. If they do not mean this, I am quite at a loss to know what they do mean, or to understand the relevancy of the so-called axiom laid down by Whitman, that "function presupposes structure," or the meaning of the statement expressed so often and with such obvious satisfaction, that "the cell is an organism". These sentences, so terse and so epigrammatic, exercise a peculiar fascination over most minds. To understand their exact applicability to the question at issue they must be carefully examined. Function presupposes structure. To the biologist who makes a rapid mental survey of his experiences, this appears to be a generalisation of universal truth. Physiology, which draws its inferences almost exclusively from the study of the higher animals, tells us that ultimately every function of the composite organism is to be referred to a particular group of cells, and that cells differ in kind according to the different functions which they exhibit. So much is this truth forced upon us that if conceivably a new function were to make its appearance, we should immediately search for the cell groups appropriate to the performance of that function. So far so good, but before proceeding further we must take note that the statement that function presupposes structure is a generalisation founded on experience. It is not an axiom as Whitman calls it, for an axiom is a proposition which is self-evident, and this assuredly is not.

The next step is to transfer this generalisation, founded on experience, into a new region, to the functions of cells. In order to do this we should possess the same experiences with regard to the functions of cells which we possess with regard to the functions of composite organisms. But these experiences are entirely wanting. We observe that protoplasm exhibits functions, that it assimilates, that it is irritable, that it is contractile, that it is reproductive, and so forth; but who has been able to demonstrate or even to suggest with any plausibility that there are structures specially devoted to assimilation, to contractility, to irritability, and to reproduction in protoplasm? It is evident that the absence of any such experiences has been felt by many observers, who have accordingly studied protoplasm with a view to finding the required structures, and some are inclined to say that the nucleus or perhaps the centrosome is reproductive, the amylum and aleurone bodies are assimilative and so forth. But there are protozoa endowed with active functions which have no centralised nucleus; the presence of centrosomes has yet to be demonstrated in protozoa in general, and there are forms in which, as Bütschli well points out, the protoplasm is homogeneous, *e.g.*, in the pseudopodia *Gromia dujardini*.[1] The reader should refer to Bütschli's work for a discussion of the subject of hyaline protoplasm (*loc. cit.*, p. 262). The fact that it exists is of the highest importance, for it shows that there is living substance exhibiting the usual vital phenomena of assimilation, contractility, etc., which, nevertheless, defies all attempts to recognise an organisation which in the light of previous experience would seem adequate to the effects produced, and it shows also that the centrosomes, the amylum grains, and their analogues, and the whole category of granules are secondary phenomena, which may be

[1] Not only are there no granules in homogeneous protoplasm, but the alveolar structure of it is unrecognisable. It is easily shown, however, that the homogeneous substance is produced from alveolar protoplasm and is capable of reconversion into it. The physical explanation of the disappearance of the alveolar structure is given by Bütschli on p. 264 of the English translation of his work.

altogether absent and yet the life processes go on unchanged.

It must be confessed then, that the experiences which so amply justified our generalisation when applied to composite organisms are altogether lacking when we seek for a justification for applying it to the simplest unicellular organisms. Moreover I have just shown that in one important particular at least, we do not merely lack these experiences, but that we have experiences of an entirely different kind. In face of this is it not obvious that the captivating generalisation must be abandoned altogether in the region which we are now discussing? For it is founded on experience, and where experience fails or is contradictory the generalisation fails also.

After what has already been said it is unnecessary for me to enter into a detailed examination of the other statement which is considered to mark a great advance in biological thought, that "the cell is an organism". It is sufficient to say that if this proposition means anything at all, it means that the cell has an organisation which is similar in kind to that of a composite organism of which a cell is a part. If I am told that it does not mean this, but something else, then I ask, firstly, what does it mean? And, secondly, if it does not mean this, what necessity is there for assuming that the protoplasm of the cell is built up of biophors, the biophor being *the* elementary living constituent, assimilating, growing and dividing, taking up definite positions in the cell, combining with others like or unlike itself to form higher aggregates, and so impressing a fixed architecture on the cell of which it is a component? Why, in short, if the statement does not mean that the organisation of the cell is the same in kind as the organisation of a composite animal, why then does everybody who believes that the cell is an organism ascribe to it an organisation which is the same in kind as that of the higher animals?

The fact is, and it is patent to everybody, that most authors do conceive of the cell-organisation as being the same in kind as the organisation of higher animals. They either have the courage of their opinions, like Wiesner,

and say so in so many words, or they tacitly admit it by their description of what they conceive cell-organisation to be. They are dominated by the cell-theory. Mr. Adam Sedgwick has recently said that the cell-theory is an incubus which perverts the minds of biologists, whose minds are so saturated with conceptions borrowed from the cell-theory that they are unable to see anything else. I have elsewhere found fault with this statement, but when the theories of cell-organisation are considered, I must freely confess that he has right on his side. Not only does the zoologist believe "that the cell is the unit of structure, and that it forms the basis of organisation in the metazoa," but he also believes that some correlative of the cell forms the basis of all organisation whatsoever. His eyes are "blinded to the most patent facts" by ideas derived from the cell-theory, and it is not too much to say that the theory does "obstruct the way of real progress in the knowledge of structure". Whether consciously or unconsciously the believer in biophors starts with ideas derived from the cell-theory, he tacitly assumes the universal applicability of the proposition that function presupposes structure, and he seeks to explain the functions of protoplasm by attributing to it an organisation which in all essential characters is the equivalent of the organisation of the metazoa. Since I have just shown that there is no justification for transferring a generalisation based upon experience to a region in which experience is either wholly wanting or, if present, of a different kind, it is hardly necessary for me to elaborate and show that it is equally unjustifiable to attribute to the unknown a plan of organisation identical in kind with the plan which we have learnt by experience to recognise as the attribute of the known.

Some time ago I pointed out that there was a fallacy in the word organism.[1] Whitman has ridiculed the statement, yet the more I reflect upon it the more I am convinced that the fallacy exists, and that it is in the highest degree mischievous and misleading. By an organism we mean

[1] G. C. Bourne, "Epigenesis and Evolution," this journal, vol i., 1894.

either an independent living thing, in which case the term is loose but applicable to every animal in the monocytial stage, or we mean a thing possessing organisation, and by organisation we mean a certain structural plan, the idea of which is a generalisation from our experience of animal and vegetable structure in general. That this is historically and in fact the connotation of the term organisation is indubitable.[1] When we use the term organisation we either use it in this connotation or in some other. If we use it in the same connotation with respect to protoplasmic structure, we are consistent, but, as I have shown, we are applying ideas derived from one set of phenomena to another set of phenomena to which they are not appropriate. But if we use it with another connotation, then we expose ourselves at once to the risk of the well-known fallacy which is inseparable from the use of the same term with different connotations. If the two connotations are clearly defined and generally understood, the fallacy may be avoided, though the inconvenience remains; but if the one connotation is clear and definite whilst the other is vague and ambiguous in the highest degree, no amount of circumspection will prevent our falling into the fallacy almost at the first opportunity. This is exactly the case with the term organisation. In the one sense we know its connotation exactly, and when authors use it in that sense they have, in the course of their arguments, to adhere strictly to the technical sense of the word. Most of them do this, for they are aware of the absurdities and inconsistencies into which they would fall if they did otherwise. But what of those who use the term with another connotation? They assure us that it does not denote a plan of structure like in kind to that of the metazoa: what then does it denote? Something so vague, so unreal and unsubstantial that we

[1] Thus in Worcester's *Dictionary of the English Language*, 1881:—

*Organisation.* The condition of an organised body or the totality of parts which constitute and the laws which regulate an organised body.

*Organised.* Formed with organs: composed of several individual parts or organs, each of which has its proper function and conduces to the existence of the entire system.

are even at a loss to know to what to apply it; its connotation has never even been attempted. The futility of using a term without connotation and with the most vague denotation is so well illustrated by the following passage from Whitman that I cannot refrain from introducing it here: "When we speak of the organisation of the germ as cut directly from a pre-existing parental organisation of the same kind we are not thinking of the definitive organisation which belongs to the fully formed organism, but of that primary organisation which belongs to the protoplasm itself". This raises our expectations, we are going to hear something of the primary organisation which belongs to protoplasm itself. Whitman continues: "We are so accustomed to connect the idea of organisation with the anatomical organs of the adult that we are apt to forget that there is a primary organisation which underlies every anatomical organ. The germ has this primary organisation; it is therefore an organism, and as such may dominate its own development." From which weighty and sententious passage we gather that the germ is an organism because it has a primary organisation which is not the definitive organisation which belongs to the fully formed organism, but a primary organisation which belongs to protoplasm itself. What on earth, we may well ask, is this primary organisation? The answer is given on the same page. It is "that original constitution of the germ which pre-determines its type of development and the form which ultimately distinguishes it from other species developing under like external conditions". The terms "original constitution" and "primary organisation" are merely synonyms. So we learn that the primary organisation so important to those who have more thoughtfully scanned the gap between the cell and the physical molecule, is *the primary organisation of the germ*, which pre-determines its type of development, etc. I hope that others are satisfied by this most remarkable piece of scientific exposition. For myself I must humbly confess that I am none the wiser for it, any more than I should be if I asked what was a Megalosaurus and I was told: "A Megalosaurus, why you know it is a big lizard, it is—a—a

Megalosaurus in fact". Nor is confusion less when I am told in one sentence that the organisation of the germ cut directly from pre-existing parental organisation of the same kind is not the definitive organisation which belongs to the fully formed organism, but is that primary organisation which belongs to protoplasm itself, and I read in the sentence immediately preceding that "the essential thing is not simply continuity of germ substance of the same chemico-physical constitution, but actual identity of germ-organisation with stirp-organisation". The organisation of the germ is identical with the organisation of the stirp, and yet the organisation of the germ is not that of the fully formed organism, but is a primary organisation which belongs to protoplasm itself. What does it all mean? It is different and yet it is identical, and it is organisation, organisation, *toujours* organisation. I beg Dr. Whitman, for pity's sake, to descend from his altitude, scarcely dreamed of in the philosophy of Harvey and Wolff, and to condescend to inform a poor bewildered mortal, who confesses to a prejudice in favour of things which he can understand, what this wonderful primary organisation is.

Seriously speaking I believe that organisation either means a plan of structure of the same type as the structure of higher animals and plants, and capable of being described in intelligible terms as it has been by Weismann, Wiesner and others, or it means nothing at all; it is a mere phrase which seeks to cover but does not conceal our ignorance.

G. C. Bourne.

(*To be continued.*)

## SOLID SOLUTIONS.

IF we define solutions as homogeneous mixtures of substances in variable proportions, we are at once obliged to admit the existence of solid solutions, for there are many mixed solids which fulfil the requirements of this definition. Common potash alum, for example, can crystallise together with ammonia alum, and form mixed crystals which are perfectly homogeneous and of the same composition through out, although the proportions of the two constituents may be varied at will by proper selection of the aqueous solution from which the crystals separate.

We are inclined, however, to look in solutions for something more than mere homogeneity and uniformity of composition, and perhaps one of the most obvious characters of a liquid solution is this, that should it at first be of different composition in different parts of its mass, there is always present the tendency of the dissolved body to attain a uniform distribution throughout the solvent. The process of equalisation of the composition, or diffusion, occurs in all solutions which are more concentrated in one part than in another, the dissolved substance moving from the place of greater to the place of less concentration. Diffusion in solution goes forward very slowly if the liquid is protected from mechanical disturbance and sudden change of temperature, months being requisite for the attainment of uniform concentration if a comparatively short column of pure solvent is placed above a denser layer of strong solution contained in the bottom of a cylinder. If diffusion takes place in solids we might expect it to proceed even more slowly.

A class of substances which form in some sort a connecting link between liquids and solids, and are specially suited to the study of diffusion phenomena, is to be found in jellies. Graham, to whom we owe our first exact knowledge of diffusion in liquids, prepared a stiff jelly containing common salt in solution in one part, and compared the rate at which

the salt diffused in it with the rate at which salt diffused in pure water. He found that the diffusion in the jelly took place almost, if not quite, as fast as in water itself. The composition of the jelly was 2 per cent. gelose and 98 per cent. water, so that, as far as actual substance was concerned, the salt had to meet practically the resistance of water alone in both cases, and the experiment showed that the mere change in apparent condition of the whole mass had little or no influence on the rate of diffusion. Subsequent experiments have served to confirm Graham's results.

When we pass to solids proper we find that instances are not wanting of what is apparently diffusion within them. Van't Hoff in his fundamental paper on solid solutions gives numerous examples. In the preparation of steel by the cementation process bars of wrought iron are packed in charcoal and subjected to a red heat for several days. The charcoal gradually penetrates the iron and converts it into steel. It matters little for our purpose what the particular form is that the carbon assumes during its passage through the iron—in some fashion or other it reaches the centre of the dense bar. The distribution of the carbon, too, if the operation is interrupted before uniformity has been attained, is precisely what would be expected if the phenomenon were one of real diffusion; and the influence of time is the same in both processes. Not only has carbon been observed to pass through iron, but it has even been proved to travel slowly through porcelain, when porcelain crucibles have been heated in a bed of graphite.

When a metal such as copper is deposited galvanically on another metal, it penetrates beyond the surface of the latter into its substance, and zinc objects which have been lightly coppered are, even when protected by a coating of varnish, occasionally observed to become white again owing to the gradual mixing of the two metals near the surface.

Professor Spring, of Liége, who has devoted special attention to the chemical behaviour of solids under high pressure, has supplied some interesting instances of pheno-

mena which can only be explained by the assumption of solid solutions. When equivalent proportions of barium sulphate and sodium carbonate are finely powdered, intimately mixed, and subjected to a very high pressure, a double decomposition takes place with formation of barium carbonate and sodium sulphate. The decomposition, however, is not complete, only 20 per cent. of the original substances being transformed. If, on the other hand, we start with a mixture of barium carbonate and sodium sulphate and compress it, we find that the reverse transformation now occurs, barium sulphate and sodium carbonate being formed, and that to the extent of 80 per cent. of the original substances present. Here we are evidently dealing with a state of equilibrium between the four substances above mentioned, which can only exist together permanently under pressure in certain definite proportions. If these proportions are departed from, the system so transforms itself that the requisite state for equilibrium is attained. Now this of itself points to the substances existing here in a state analogous to that of bodies in liquid solution, for we know that in general definite proportions are necessary in solutions for stable equilibrium to exist. In the case of solids the general rule is that when they are in equilibrium under given conditions in one proportion, they are in equilibrium under the same conditions in every other proportion. The behaviour, then, of these solids under pressure is analogous to the behaviour of substances in solution, and different from the ordinary behaviour of solids. The continuance of the pressure is not essential to the establishment of such a definite solid equilibrium, for Spring has shown that by relieving the pressure after 73 per cent. of a system of barium carbonate and sodium sulphate had been transformed, the process continued, though less rapidly, and after a week had reached the proportion of 80 per cent. necessary for equilibrium. Here diffusion must have played a part, for no matter how finely divided the reacting substances originally were, their surface of contact (where alone the mutual decomposition could take place if there were no diffusion) must have been comparatively small.

It is well known that some metals have the property of allowing certain gases to pass through them under favourable conditions, the most thoroughly investigated instance of this kind being the permeability of the metal palladium to gaseous hydrogen at moderately high temperatures. At about 300° C. hydrogen can pass quite freely through a palladium septum, and it is difficult to conceive the nature of this phenomenon without admitting the existence of diffusion in the solid. Whether the hydrogen is dissolved in the palladium or forms a compound with it, as has been asserted, is of little consequence, for in the latter case the compound superficially produced must have possessed the power to penetrate the remaining metal, or to allow of the passage of hydrogen through itself.

Connected with the process of diffusion in solution we have the phenomena of the conduction of electricity in solutions, or electrolysis. Here the electric current is carried by material particles, and the resistance that these experience in their passage through the solution is of the same nature as the resistance offered to diffusion. Helmholtz, in his Faraday lecture, drew attention to the fact that glass behaves as an electrolyte towards an electric current, *i.e.*, that the current in passing through the glass is associated with two currents of particles moving in opposite directions. The particles travelling towards the negative pole of the battery have since been proved to move faster than those moving towards the positive pole. Lehmann also has shown that when two silver electrodes are immersed in fused iodide of silver, which is afterwards allowed to solidify, and a current of electricity is passed through the solid iodide, one of the electrodes increases in weight at the expense of the other, and that the phenomenon can be reversed by reversing the current.

These examples will suffice to indicate that we are not without data to establish an analogy between the behaviour of certain solids and the behaviour of ordinary liquid solutions. Since the appearance of van't Hoff's original paper on the subject a considerable number of researches have been published more or less directly bearing on the question,

but the results achieved have on the whole been small, owing chiefly to the experimental difficulties encountered.

An important application of the idea of solid solutions was made by van't Hoff in explaining the abnormalities that are sometimes met with in the determination of molecular weights by the lowering of the freezing-point in solutions. It had been proved theoretically that the freezing-point of a given solvent should be depressed to a certain value (calculable from the freezing-point and the latent heat of fusion of the solvent) when the solution was of normal concentration, *i.e.*, contained one gram-molecule of dissolved substance per litre. The nature of the dissolved substance should be without influence on this value. Now, whilst it was ascertained experimentally that this theoretical relation was in the vast majority of cases accurately fulfilled, yet there remained certain combinations of dissolved substance and solvent which gave values of the depression constant altogether at variance with the calculated value. Thus, metacresol dissolved in phenol gave a depression of 48 instead of 74, and thiophene dissolved in benzene a depression of 34 instead of 53. Van't Hoff's explanation of these and similar abnormally low values of the depression was that the freezing-point observed was not in the strict sense the freezing-point which had been assumed in the theoretical reasoning. The true freezing-point of a solution is the temperature at which the liquid is in equilibrium with the *solid solvent*. The freezing-point of an aqueous salt solution, for example, is the temperature at which it can exist in contact with pure ice without the ice melting or without fresh ice being deposited from the solution. Now, in the exceptional cases above alluded to it is known that the solid and the solvent have a tendency to crystallise together, *i.e.*, to form mixed crystals, so that the substance that separates out is not the pure solvent but rather a solid solution. The temperature at which such a solid solution would be in equilibrium with the liquid solution might not by any means be the freezing-point of the solution as above defined. The apparent observed freezing-point of the solution, therefore, would not in general coincide

with the calculated depression, and van't Hoff from theoretical considerations showed how the divergence could be estimated from a knowledge of the composition of the solid which actually separated out from the solution on cooling. That the abnormal values for the points of solidification depend on the separation of the dissolved substance along with the solvent has now been experimentally verified in a considerable number of cases. Heycock and Neville found that for the case of solutions of antimony in molten tin, the freezing-point of the tin was raised instead of lowered by the presence of the second metal. Küster has shown that this and similar instances are susceptible of a very simple explanation. The two metals separate out together in very nearly the same proportion as that in which they remain behind in the liquid, so that the solution solidifies as a whole. In such circumstances the point of solidification of the liquid can be calculated by the simple mixing formula. If the melting-point of each pure substance is multiplied by the proportion in which it exists in the mixture, the sum of the two numbers thus obtained will give the point of solidification of the solution. As antimony melts 200 degrees higher than tin, the admixture of the former in however small proportion will, since the mixture freezes as a whole, raise the point of solidification instead of lowering it, as would be the case if pure solid tin separated from the liquid on cooling.

Not only do solutions exhibit a lower freezing-point than that of the pure solvent, but they also exhibit a lower vapour tension. The pressure of aqueous vapour over salts containing water of crystallisation may in many cases be measured with accuracy, and there it is found that the isomorphous admixture of another salt lowers the vapour pressure of water which is in equilibrium with the solid. Thus the vapour tension of a mixed crystal of ordinary alum with iron alum is less than the vapour tension of either of its components. In this respect then the mixed crystal behaves as a solid solution. Again, the solubility of a substance is diminished when it itself acts as a solvent for another substance insoluble in the original solvent. Of the

three liquids, ether, water, and benzene, ether and water are partially miscible, benzene and water are immiscible, and ether and benzene miscible in all proportions. Suppose we take water as the original solvent—then on shaking it up with ether we find that the latter dissolves to a certain definite extent in it, *i.e.*, possesses a certain solubility in water. If now we previously dissolve benzene in the ether which we shake up with the water, we find that the water will now take up less ether than before. The solubility of ether in water is thus diminished when benzene is dissolved in it—and this behaviour is characteristic of all such combinations of substances.

A case of this kind where two solids play the part of the ether and benzene in the previous instance has been thoroughly studied by F. W. Küster. The solid hydrocarbon naphthalene is, like the hydrocarbon benzene, insoluble in water; $\beta$-naphthol, on the other hand, is, like ether, sufficiently soluble in water to permit of accurate estimation. But naphthalene and $\beta$-naphthol can crystallise together in any proportion so as to form a complete series of isomorphous mixtures, the melting-points of which vary according to the rule given above for mixtures of antimony and tin. A comparison of the amount of $\beta$-naphthol dissolved by a given quantity of water from such mixtures led to somewhat unexpected results. Instead of the addition of a small quantity of naphthalene to $\beta$-naphthol lowering the solubility of the latter in water, it was found that mixtures containing as much as 30 per cent. of naphthalene had precisely the same solubility as $\beta$-naphthol itself. As more naphthalene was added the solubility increased slightly, afterwards to diminish continuously to zero as the mixture was made to contain more and more naphthalene. The explanation of this behaviour suggested by Küster is that naphthalene and $\beta$-naphthol are capable of forming a chemical compound consisting of one molecule of each substance, this compound being decomposable by water, an assumption by no means improbable, as many similar cases have been observed. If we allow further that the solubility of the compound is greater than the solubility of $\beta$-naphthol, the

results are satisfactorily accounted for. The diminution of solubility when much naphthalene is present is the normal depression of the solubility of the compound by the addition of excess of naphthalene. The solubility greater than that of pure naphthol is the solubility of the compound naphtholnaphthalene. The constant solubility (equal to that of $\beta$-naphthol) observed when there is little naphthalene in the mixture is the solubility of $\beta$-naphthol, for the naphthalene in the mixture is in the form of the compound naphtholnaphthalene, which is decomposed at the surface by water into naphthalene and $\beta$-naphthol, which exist now alongside of each other and not in the intimate union of a crystalline isomorphous mixture.

In connection with the results of these experiments Küster is inclined to make a distinction between crystalline isomorphous mixtures and solid solutions proper, because in the former there is practically no diffusion owing to what may be termed the rigidity of the crystalline structure. He admits, however, that no absolutely sharp line can be drawn, as there are various intermediate degrees in which diffusion may take place. A reference to the examples of diffusion in solids previously cited in this paper will show that they all occur in amorphous bodies without any regular structure.

A point of considerable interest in the theory of solid solutions is that it affords us the possibility of determining molecular weights of the dissolved substances, and since in isomorphous mixtures we usually attribute similarity of molecular structure to the two components, we can also in this case form an estimate of the molecular weight of the solid solvent. From his experiments on the amount of $\beta$-naphthol dissolved by water from mixtures of that substance with naphthalene, Küster was able to calculate with a high degree of probability the molecular weight of each of these substances in the solid state. In the first place he found that with mixtures containing excess of naphthalene the ratio of the square root of the concentration of $\beta$-naphthol in the solid mixture to the concentration in the aqueous solution saturated by that mixture was very nearly constant,

varying but little with the actual composition of the mixtures taken. The general theory of solutions asserts that when a substance (here $\beta$-naphthol) is divided between two immiscible solvents (here water and naphthalene, or naphtholnaphthalene) it will be distributed in a constant ratio between the two solvents, no matter what amount of it be taken, provided only the molecular weight of the substance is the same in both solvents. In the case investigated this does not hold—the ratio of the concentrations in the two solvents is not constant; and the molecular weight of $\beta$-naphthol dissolved in water is therefore different from the molecular weight of $\beta$-naphthol "dissolved" in naphthalene. The theory further asserts that when, as in the present instance, the concentration in one of the solvents is proportional to the square root of the concentration in the other solvent, the molecule in the second solvent must be twice as great as the molecule in the first. We know that $\beta$-naphthol dissolved in water has the normal molecular weight corresponding to the formula $C_{10}H_8O$; in naphthalene solution it has consequently the molecular weight corresponding to the formula $(C_{10}H_8O)_2$.

The theory of solutions likewise enables us to calculate the molecular weight of the naphthalene in the above experiments from the diminution of the solubility of the $\beta$-naphthol in water as it dissolves more and more naphthalene. In the case before us the question is slightly complicated by the existence of naphtholnaphthalene molecules, but Küster was able to arrive at the result that naphthalene must have double the molecular weight in the state of solid solution that it has in the state of vapour, *viz.*, $(C_{10}H_8)_2$.

Another well-investigated case of solid solutions is that offered by the absorption of hydrogen by palladium. Troost and Hautefeuille, in order to obtain information as to the state in which the hydrogen existed within the metal, made an extensive series of observations of the pressure of hydrogen in equilibrium with palladium containing different amounts of hydrogen. They found that with compositions of the solid up to one atom of hydrogen to two atoms of palladium the pressure of hydrogen remained constant

at 100° C., after which it increased rapidly as the proportion of hydrogen in the solid increased. The analogy between this case and the case of the solubility of mixtures of $\beta$-naphthol and naphthalene in water is at once apparent. In both instances we have constancy of pressure (gas-tension) and solubility (solution-tension) within a certain range of composition, and then rapid variation with further change of composition. The conclusions arrived at in both instances are also similar. The constant solubility was attributed by Küster to the formation of a compound naphtholnaphthalene—the constant tension was attributed by Troost and Hautefeuille to the formation of a compound $Pd_2H$, in which any excess of hydrogen was then absorbed. Quite recently, however, grave doubts have been thrown on the existence of this compound. A very careful repetition and extension of Troost and Hautefeuille's experiments by C. Hoitsema has proved that the constancy of tension observed by these investigators was not absolute but only approximate, and that under slightly varying conditions the apparent constancy disappeared altogether. It would seem, therefore, that no compound of palladium and hydrogen is formed when the gas is absorbed by the solid, the state of the hydrogen being rather one of simple solution in the palladium. A comparison of the concentrations of the hydrogen above the palladium and of the hydrogen in the palladium indicates that at very low pressures the hydrogen in the metal exists as molecules only half as great as those of the gas, *i.e.*, as molecules consisting of only one atom. At higher pressures the concentration of the free gas and that in the palladium stand in a nearly constant ratio, from which it is to be inferred that the molecule of hydrogen in the metal, as well as the molecule of gaseous hydrogen, is represented by the formula $H_2$.

A problem which has long interested chemists is the determination of the nature of the process involved in dyeing. Some contended that the process was one of chemical union of the dye with the substance of the fibre, others that it was merely one of mechanical absorption. In 1890, however, O. N. Witt propounded a new theory which, on account of

its plausibility, met with a ready acceptance in many quarters.

According to Witt the state of the dye-stuff in the fibre is one of solid solution, and many analogies were advanced in support of this assertion. For example, dyed materials show the colour, not of the solid dye-stuff, but of the dye-stuff in solution, when there is a difference of colour between the two states. Solid fuchsine is green, its aqueous solutions are red, and so also are materials dyed with it. The dye-stuff rhodamine in the solid state exhibits no fluorescence, in solution it does, and silk dyed with rhodamine is fluorescent likewise. The theory of Witt thus appeared very promising as an explanation of the phenomena of dyeing, but a closer investigation has shown that it cannot be accepted unconditionally, although some modification of it may be found to satisfy the experimental requirements. It has been proved in a considerable number of instances now investigated that the concentrations of the dye in the dye-bath and in the fibre do not stand to each other in a relation of simple proportionality, but the concentration in the bath is roughly proportional to a power (usually 3 to 5) of the concentration in the fibre. Now on the theory of solid solutions this indicates that the molecule of the dye in the water is three to five times as great as the molecule of the dye in the silk; but this cannot be the case, for the molecule of the dye-stuff in aqueous solution can be shown by other means to be the simplest possible. The numbers rather indicate analogy to the process known as absorption from solution. Substances like animal charcoal and platinum black have the property of condensing gases in the extensive surface they present. Similarly they can abstract certain substances from solution, as may be seen in the employment of animal charcoal for the decoloration of solutions. The relation between the concentration in the solution and that in the charcoal proves to be of the same kind as is met with in dyeing, so that we are led to suspect a similarity in the nature of the two processes. The so-called "iodide of starch," the blue compound formed when starch and iodine solution are brought into contact, would

appear to be a substance of the same nature as a dyed fibre and as charcoal saturated with an acid from solution, for the concentrations of the iodine in the aqueous solution and in the starch obey approximately the same law as in the other instances.

We are therefore forced to conclude that whatever success has attended the application of the theory of solid solutions to other processes, the theory can scarcely without modification be accepted as giving an explanation of the process of dyeing.

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JAMES WALKER.

# THE STELAR THEORY; A HISTORY AND A CRITICISM.

## PART I.

IN the study of the histological anatomy of plants, apart from the structure of the individual cell, the greatest advances of the last two decades have been made rather by the establishment of new points of view than by the discovery of new facts. Twenty years ago the solid foundations of the subject had been securely laid, and a considerable portion of the imposing fabric of histological detail which now rests upon them had already been built up. This fact is most clearly brought out by the masterly summary of existing anatomical knowledge published by De Bary in 1877. But splendid monument as it is of its author's unsurpassed knowledge of his subject, there can be few who have not felt that the *Vergleichende Anatomie* is, as a whole, essentially unreadable. Compare it, in imagination, with Sachs' *Vorlesungen* or with Haberlandt's *Physiologische Pflauzenanatomie*, and we are forced to recognise that De Bary's work is rather an encyclopædia than a piece of great scientific literature. The cause is to be found in the simple fact that there did not exist in 1877 a philosophy of the morphological aspect of the subject capable of informing "an epitome of the present knowledge of 'the Anatomy of the Vegetative Organs of Vascular Plants,'" as the idea of adaptation informed the works of Sachs and Haberlandt.

It is nothing less than the establishment of such a philosophy that we now owe to the great Frenchman, Van Tieghem. The most important part of his ideas is contained in what we may call the Stelar Doctrine of Vascular Tissue, and it is with this that we shall here be exclusively concerned.

Although the foundations of the stelar theory were laid many years ago, outside France it has made its way very slowly. In Germany even now it is apparently ignored

by the majority of anatomists, notwithstanding its acceptance by the most brilliant of German contemporary investigators.

In England, though these ideas have recently been made familiar to the student by more than one of our leading botanists, their discussion has still the interest of comparative novelty. And although the general idea of the stele as a morphological unit is simplicity itself, yet the application of this idea is in some cases by no means easy, so that not only does Strasburger's interpretation of certain facts differ from Van Tieghem's, but the author of the theory has himself been led to modify his original views in an important manner. The possibility of such a difference in the interpretation of facts which are undisputed seems to spring, if we may say so without presumption, from a certain want of definiteness in the apprehension of the criteria legitimate to their interpretation.

To investigate these criteria and to endeavour to ascertain their relative validity is one of the primary objects of the present paper.

We shall begin with an account of the development of the stelar doctrine.

## HISTORY OF THE STELAR DOCTRINE. FIRST PHASE—THE IDEA OF THE CENTRAL CYLINDER.

In 1870-1, Van Tieghem published, in the *Annales des Sciences Naturelles*, a memoir (1) which was to have been the first of a series entitled "Recherches sur la symétrie de structure des plantes vasculaires". This instalment consisted of a general introduction setting forth the plan of the whole work, followed by 274 pages devoted to an extended anatomical account of the root, in vascular plants.

The introduction is of the greatest interest. The author tells us how he wished to obtain anatomical definitions of root, stem, and leaf, in order to give a basis to the study of comparative anatomy. These definitions are to be framed in accordance with the different kinds of symmetry exhibited in the arrangement of the vascular strands in the three organs, to each of which a separate memoir

is to be devoted. The results so obtained are to be applied, in a further series of memoirs, to the solution of a number of morphological problems, such as the true nature of tendrils, tubers, spines, phylloclades, ovules, etc., and finally, to the elucidation of the laws of symmetry governing the structure and relations of the ideal colony that would be formed if every seed germinated *in situ*.

This elaborate scheme for "un cercle d'études anatomiques complets et fermés" enables us to understand the strength and the weakness of the author's stelar theory. The imperative desire to reduce the anatomy of vascular plants to a perfect system depending upon simple laws of symmetry governing the arrangement of the vascular tissue, has been the means of giving us a doctrine, luminous indeed, and of wide significance, but scarcely of that rigidly universal application which its author claims. But here again, as is so often the case in the history of science, the attempt to work out logically the various implications of such a theory, has been of the utmost value in clearing our ideas and extending knowledge, not only by stimulating to the discovery of new facts, but by forcing us to examine the foundation of our conceptions.

Of Van Tieghem's scheme, as it stood in 1870, however, only the first memoir, that on the root, was ever written. The author demonstrates the fundamental identity of structure in the roots of all vascular plants, and obtains his anatomical definition based on the symmetry of the vascular system. He shows that the vascular tissue of a young root forms a central cylinder which contains near its periphery "faisceaux libériens" (phloems) alternating with "faisceaux vasculaires" (xylems) united by "cellules conjonctives". Hence the vascular system is symmetrical in relation to a *line*, which is the organic axis of the organ. The stem agrees with the root in this last point, but on the other hand has its "faisceaux libéro-vasculaires," "réunis directement par le parenchyme primordial". Where the main root passes into the main stem there occurs a "cessation du tissu conjonctif spécial, qui se trouve remplacé par le parenchyme primitif". This sentence is specially interest-

ing because it shows that when it was written Van Tieghem had no idea of a central cylinder in the stem.

Two years later, however, in 1872, in describing (2) the transition from root to shoot in *Tagetes patula*, he writes how the "membrane protectrice" (endodermis) is continued up into the stem, retaining its characteristic thickenings, and immediately internal to it the "membrane rhizogène" of the root (later named the *pericycle*) is also found in the stem still giving rise to rows of lateral roots, one row arising from each interval between two bundles. Opposite the bundles, however, he holds that the "membrane rhizogène" is interrupted, since here the endodermis abuts directly on the group of fibres capping the bundle, fibres which in accordance with the current opinion he considered to belong to the phloem. Here then we have the first clear description of the continuation of the central cylinder of the root into the stem, and the idea of this continuation is the fundamental idea of the stelar theory. It is most clearly expressed in a note on p. 112, "Ainsi, et j'insiste sur ce point, la tige est, comme la racine, et dans toute son étendue, composée d'un cylindre central et d'un parenchyme cortical limité en dehors par un épiderme, en dedans par une membrane protectrice ou endoderme".

The generality of this condition is further insisted upon: "Le caractère sur lequel je viens d'appeler l'attention se retrouve dans la tige de la grande majorité des plantes vasculaires, mais il souffre pourtant quelques exceptions. M. Caspary a montré, en effet, que dans quelques plantes (*Minyanthes trifoliata*, *Adoxa moschatellina*, *Brasenia peltata*) chaque faisceau constitutif de la tige est individuellement entouré par une membrane protectrice à cellules plissées ('Bemerkungen über die Schutzscheide,' in *Pringscheim's Jahrbücher*, 1865-66, iv., p. 101). J'ai retrouvé le même fait sur quelques autres plantes, notamment sur *l'Hydrocleis Humboldtii*. Dans ce cas, il n'y a pas non plus de membrane rhizogène dans les entrenœuds de la tige, et il n'existe aucune solution de continuité, aucune distinction réelle entre le parenchyme cortical et la moelle" (p. 113). This paragraph shows clearly that thus early

Van Tieghem had recognised the condition which he afterwards described as "astely".

The "membrane rhizogène," now considered, under the name of pericycle, as forming merely the external layer of the conjunctive tissue of the cylinder, was at that time treated as a region external to, and distinct from, the rest of the parenchyma, to which the name "conjonctif" was given. But the clear recognition of the existence of an individualised stem cylinder, forming a direct continuation, tissue for tissue of that of the root, was the first and fundamental step in the evolution of the stelar idea.

Little progress was made during the next ten years in the development of this conception.

Falkenberg (3), in 1876, showed that the "Aussenscheide" in monocotyledonous rhizomes corresponds with the "pericambium" in roots, both in position and *rôle;* and Mangin (4) in 1882 entirely confirmed his results and showed that not only adventitious roots but also the "réseau radicifère" arises from this layer, which he calls the "couche dictyogène".

In 1882 Van Tieghem published a short paper (5) in which, *à propos* of the Cucurbitaceæ, he gives conclusive reasons, based upon grounds of comparative anatomy, for regarding the fibres in the stem, hitherto called primary "bast fibres," as really belonging to the "membrane rhizogène". With these extended limits, this layer forms a complete investment of the stem cylinder, just as the pericambium does of the root cylinder. Since the one layer is the direct continuation of the other, and the two correspond very largely in function as well as in position, it is clearly desirable that they should have a common name. For this purpose Van Tieghem introduced the word *pericycle*, which was to supersede the various terms "pericambium," "Aussenscheide," "membrane rhizogène," "couche dictyogène," etc., applied by various writers to the same layer in various plants and parts of plants, according to its various histological characters and functions. The importance of this introduction of the conception of the pericycle was of course very great, since it fixes more accurately the external

limit of the cylinder, and thus brings into greater prominence the idea, already clearly stated in 1872, of an individualised stem cylinder in direct continuity with that of the root.

The term has eventually, though very slowly, found its way into general use.

In 1884 Morot, a pupil of Van Tieghem, published the results of a research (6) devoted to a comparative investigation of the pericycle in both root and shoot.

The publication of Morot's paper brings to an end what we may call the first phase in the development of the stelar idea.

## SECOND PHASE—POLYSTELY AND ASTELY.

The second phase was inaugurated in an investigation (7*a*) by Van Tieghem and his pupil Douliot, of the anatomy of the stem of various species belonging to the genus Primula. Their observations were carried out on a number of new species from the East, as well as on many old species, making together a total of 114. They resulted (7*b*) in a division of the aggregate genus *Primula* L., into two segregates, *Primula* Tourn. and *Auricula* Tourn., as had been already done by Tournefort, but now based on a fundamental difference in the structure of the stem of the two segregate genera. While the stem of the species belonging to *Primula* possesses a single normal central cylinder in its whole extent, the narrow cylinder of the hypocotyl of an *Auricula*, instead of dilating in the ordinary way above the level of the cotyledons, gives rise by successive bifurcations to two or more vascular strands, each surrounded by an endodermis and possessing the structure of the single hypocotyledonary cylinder. In the genus *Gunnera* (*Halorageæ*) a similar state of things obtains. These facts were, in the main, already known, having been investigated by Vaupell, Kamienski and Reinke. The opinion of these authors was, however, that the separate vascular strands were vascular bundles of the "concentric" type with peripheral phloem, comparable for instance to those found in the cortex of certain *Melastomaceæ;* and this was the view

taken by De Bary in his classical *Vergleichende Anatomie.* The numerous vascular strands in the rhizomes of most Leptosporangiate Ferns were regarded by De Bary in the same light.

But Van Tieghem, having, as we have seen, come to regard the central cylinder rather than the bundle as the morphological unit of vascular tissue in both root and shoot, was now led to the conclusion that in *Auricula, Gunnera* and the majority of Ferns[1] we have really to deal with a splitting of the single cylinder of the hypocotyl, as we trace it upwards, by successive bifurcations, into a number of such cylinders (7*c* and 8). Van Tieghem and Douliot proposed to call such a cylinder a *stele* (Greek στήλη, a column). A root or a stem containing one such stele would be *monostelic*, if it contained more than one *polystelic.* A third case was distinguished. If the cylinder of the hypocotyl breaks up, as it is traced upwards, into its component bundles, each of which is surrounded by a special endodermis, the cylinder, according to our authors, no longer exists; the stem is *astelic.* This case, already described in 1872, obtains in the stems of various *Ranunculaceæ*, in *Nymphæaceæ*, in *Hydrocleis*, in some species of *Equisetum*, etc., as well as in the majority of petioles and in blades of all leaves.

Cases of Polystely fall into two groups. First, where on a transverse section the various steles are seen to be completely separate, we have a state of *dialystely.* Secondly, where the steles are united laterally, so as to form a more or less complete ring in transverse section, enclosing a more or less isolated portion of extra-stelar tissue, which occupies the centre of the ring, we have a state of *gamostely.* These two conditions are not to be sharply separated, since the steles of all polystelic stems show more or less frequent lateral unions, and the gamostelic condition is simply a case where these unions are very frequent and persistent.

[1] Leclerc du Sablon in 1890 (9) worked out the connections, in several Ferns, of the single hypocotyledonary cylinder with the cylinders of stem.

We may tabulate the results thus obtained as follows:—

*Monostely.*—A single central cylinder. All roots and hypocotyls, nearly all Phanerogamic stems, and stems of many Vascular Cryptogams.

*Polystely.*—More than one cylinder. Stems of most ferns, most species of Selaginella, and among Phanerogams of *Auricula* and *Gunnera.*[1]

(*a*) *Dialystely.*—Steles separate for most of their course. Most Ferns *Selaginellæ* and. *Auricula ursi*, etc. *Gunnera.*

(*b*) *Gamostely.*—Steles united laterally for most of their course. *Marsilia*, *Pilularia*, *Pteris aurita*, etc. *Auricula japonica*, etc.

*Astely.*—No cylinder. Leaf blades, most petioles, stems of some species of *Equisetum* and *Ranunculus*, stems of *Hydrocleis*, *Ophioglossum*, *Limnanthemum*, *Nymphæaceæ*, etc. (7*c*).

The publication, in 1890-91, of the second edition of Van Tieghem's *Traité de Botanique* (10), which contains a full exposition of the stelar doctrine on the lines indicated, may be said to mark the close of the second phase in the development of the theory.

## THIRD PHASE—EXTENSIONS AND MODIFICATIONS.

The third phase, from that date to the present time, has been occupied by various developments and modifications of the doctrine on the part of the author and his pupils, and has been marked by considerable criticism, mainly of these newer developments.

The first line of research that calls for notice is a re-investigation of the conjunctive tissue of the typical central cylinder of the flowering plant. This has led Flot (11) to

[1] In a paper recently communicated to the Linnean Society, Mr. B. G. Cormack describes cases of polystely met with in the adventitious roots of three genera of Palms, *viz.*, *Areca*, *Cocos* and *Verschaffeltia*. It appears that the single stele of the root splits, as it is traced downwards, into a ring of separate steles. Later on these steles again pass over into a single cylinder. This seems to be an important modification of Polystely as described by Van Tieghem and Douliot, and Leclerc du Sablon.

add a new region to those already distinguished. He finds a zone situated at the periphery of the pith, *i.e.*, just internal to the ring of bundles, corresponding exactly to the pericycle external to the ring, as well characterised histologically as the pericycle itself, and indeed resembling the latter very closely in structure and *rôle*. This zone, the *perimedullary zone*, is according to Flot (and his figures entirely support this) separate in development from the pith proper, or *internal conjunctive*, and belongs rather to the hollow cylinder of tissue (the "thickening ring" of the older German anatomists) giving rise to the bundles and the conjunctive immediately surrounding them (*external conjunctive*). It is impossible sharply to separate the perimedullary zone on the one side, just as Morot found it impossible to separate the pericycle on the other, from the ray tissue, and we should rather regard the contrast of the pith with the external conjunctive tissue, as of greater importance than the division of the latter into pericycle, rays and perimedullary zone, which are in the main *topographical regions* marked out by the limits of the bundles. In many adult stems it is however impossible to fix the limits of external and internal conjunctive, just as it is often impossible to fix the limits between external conjunctive and cortex. Flot is of opinion that this is owing to a growth in breadth of the cells of the external conjunctive continued longer than in the pith, the whole of the tissue of the cylinder thus becoming approximated in size and shape. This same cause, together with a masking of the endodermal thickenings (in cases where these are originally present) by a general thickening of the walls of all the parenchyma cells may very conceivably account for the frequent absence of the obvious limit between cortex and cylinder, though we are not aware that such an occurrence has been either established or suggested.[1] Further investigation on this point, as well as on the separation of the regions in root cylinders with a well-developed con-

[1] I now find that Sanio (24, pp. 371-2) states that this is practically what occurs in the stem of *Ranunculus acris*.

junctive system, is much needed to complete our knowledge of these matters.

An important modification of the theory of steles has been made by Van Tieghem himself in extending the use of the term astely so as to make it include the state of things obtaining in the stems of all species of *Equisetum* (12), and of *Ophioglossaceæ* (13).

Let us take first the case of *Equisetum.* Well-marked endodermes are found in the stems of all species, but their disposition, which was fully worked out many years ago by Pfitzer, is very various, not only in different species, but in different parts of the stem of the same species. There are three types of arrangement. In the first each vascular bundle is surrounded by a special endodermis ; in the second the ring of bundles is bordered within and without by a general endodermis ; and in the third the outer endodermis alone is present. In the second edition of the *Traité* Van Tieghem assigned the first two conditions to the astelic, the third to the monostelic type, but in a paper (12) published in the same year (1890) he calls attention to the fact, discovered by Pfitzer, that *all the species possess, at their nodes, the first or second of the arrangements in question.* He therefore concludes that all belong really to the astelic type, and that where, for instance, the second type, just above a node, passes back into the third, we have simply a case of the disappearance of the special characters of the inner endodermis, *which must still be supposed to exist.* The "monostely" is only apparent, and the tissue bordering the central canal of the stem, internal to the inner (theoretical) "endodermis," is not in reality pith, but rather "inner cortex" (extra-stelar tissue). The first of the three arrangements is to be called *dialydesmic*, since each bundle with its sheath of conjunctive is separate ; the second and third *gamodesmic*, since the conjunctive tissue surrounding the bundles is in lateral confluence.

Turning now to the *Ophioglossaceæ* we have a similar argument (13). The stem of *Ophioglossum vulgatum*, below the level of the first leaf, is monostelic, but above the

first leaf contains five separate bundles each with a separate, though feebly suberised, endodermis. Hence it was treated by Van Tieghem, in the *Traité*, as astelic. In *Botrychium Lunaria*, whose stem is also monostelic at the base, the endodermis, after the departure of the first leaf trace, does not close round each separate bundle but becomes as it were invaginated into the cylinder, so that the vascular tissue forms on transverse section a horse-shoe bounded by the endodermis. The free edges of the horseshoe meet, as we pass up the stem, and the inner portion of the endodermis becomes entirely separated from the outer, so that we have an equivalent of the second or gamodesmic condition found in the stems of *Equiseta*. Higher up the inner endodermis loses its thickenings, just as in some *Equiseta*, and this gives us an apparently monostelic condition. In accordance with his revised view, Van Tieghem considers that *Ophioglossum* has an astelic-dialydesmic stem, while those of *Botrychium* and *Helminthostachys* are astelic-gamodesmic.

## THE STATUS OF THE STELE CRITICISM.

It will be most convenient to introduce here a critical investigation of the stelar theory as thus modified by its author, and so far as it depends upon the morphological interpretation of the arrangement and relations of vascular tissue in the adult organs of vascular plants; deferring for the present a consideration of the developmental facts bearing upon the theory.

There is no need to discuss at any length the fundamental conception of the stele arrived at in the period which we have called the first phase of development of the idea. It depends upon the tracing into the stem of the root cylinder, and upon the demonstration that its characters as a cylinder are maintained in the latter. This demonstration, begun, as we have seen, in 1872, eventually led to the explicit recognition of the fact that the system of bundles forming the central cylinder possesses morphological characters much more constant than those of the vascular bundle,

and is hence more worthy to be taken as the morphological unit of vascular tissue. It is indeed impossible to give a morphological definition of a vascular bundle at all. "From the very first those bundles which consist essentially of definitely arranged groups of tracheæ and sieve tubes . . . have been called *vascular bundles*" (14, p. 232, Eng. ed.). But thus defined, a "vascular bundle" has no constant histological characters beyond the fact of containing both xylem and phloem. According to the arrangement of these, bundles have been classified as radial, concentric, collateral, etc. Such an arrangement brings together vascular strands of very different orders of complexity. In the first place it associates the axial cylinder ("radial bundle") of a root, possessing a number of quite distinct xylem and phloem strands, with the "collateral bundle" of a Phanerogamic stem, formed of a single strand of xylem and phloem in close association, the latter being continuous moreover with a portion only of the former. Again it associates even more closely under the term "concentric bundle" the vascular strands found in the stem of *Auricula*, *Gunnera* and Ferns with those of quite different structure found in the pith and cortex of *Melastomaceæ*, etc.

Such a classification is clearly, from a morphological point of view, quite artificial. But if we extend the use of the term bundle, as is often done, so as to include strands of tracheæ alone, and of sieve tubes alone, we can retain it as a convenient word without morphological connotation, and applicable to any strand of tissue belonging to the vascular system. And we may then qualify the word by any adjective we choose without morphological implication. Thus we may speak of the composite radial bundle of the root as composed of separate xylem bundles and phloem bundles alternating at its periphery; of the concentric bundle of the stem of an aquatic plant as sometimes composed of separate collateral bundles, in other cases consisting simply of a continuous cylinder of phloem surrounding a central strand of xylem; of the concentric bundle of a fern petiole gradually passing to the collateral type as we trace it into the lamina, and so on. Meanwhile the study of the homologies of the

various strands is quite a distinct matter, and requires a distinct terminology.

## THE BOUNDARY OF THE STELE.

The acceptance of the central cylinder in the "monostelic" stem as a region of the first morphological rank is now very general. The only criticism which we have to consider is that which calls attention to the frequent want of definiteness about its external limit, and is inclined on this ground to question its individuality. This want of definiteness arises from the absence, in many adult stems, of the special characters of the endodermis (innermost layer of the cortex), often combined with an identity in size, shape and characters of cell-membrane between the cells of the cortex and those of the conjunctive. Such a state of things obtains, to take a single instance, in the stem of *Ranunculus repens*. A transverse section of such a stem shows the separate bundles imbedded in a homogeneous ground tissue, and to speak of a well-marked central cylinder is to speak of that which does not, in fact, exist.

Now this, as it stands, is a perfectly legitimate criticism, and its force as against the general validity of the stelar idea depends simply upon the greater or less generality of the condition described. Van Tieghem (10, p. 752) states that when, after the formation of the endodermis, the stem undergoes considerable intercalary growth, the folds on the radial walls of the endodermal cells become stretched out so that they become difficult or impossible to see. In other cases no suberisation of the radial walls occurs, and then, unless the endodermal cells are distinguished by possessing starch, it is admitted that the limit of the cylinder is difficult to determine, but says Van Tieghem (*loc. cit.*): "il reste la forme différente des cellules". This, however, as has been said, is by no means always obvious. A possible cause of such a condition, assuming the limits of the *young* cylinder to be well defined, has already been suggested, but new investigations are necessary to determine the point. If, for the sake of argument, we make the opposite assump-

tion, that the vascular bundles are sometimes differentiated in the middle of a homogeneous ground tissue, no trace of a special endodermis or pericycle being visible at any time, we could not predicate the existence, *in such cases*, of a central cylinder in the stem. And further, if such a condition obtained in the majority of instances (certainly an unlikely supposition) we should not, of course, be justified in predicating the *general* existence in the stem of a central cylinder, and this would necessitate such a radical modification in the generalised statement of the facts, that the stelar idea would lose the greater part of its significance. We shall have to recur to a discussion of the limit of the cylinder, but these simple considerations are insisted upon here, because they are apparently lost sight of in much of the current writing of Van Tieghem's adherents. It seems to be implicitly assumed that if a good anatomical distinction can be made in a certain number of cases, it is permissible to generalise the distinction and erect it into a morphological doctrine. The existence of those cases to which the doctrine does not apply is either ignored, or the distinction is said to be "theoretical". There is of course no such thing as a true "theoretical" distinction which is not also actual. The fallacy arises from a tendency to regard all morphological doctrine as of absolute value, whereas its value is never anything but relative. What we have to decide in any given case is the *amount* of this relative value, and whether that amount is sufficient to make the doctrine express a general truth so far as the objects under consideration are concerned.

The foregoing reflections lead us naturally to consider those cases which Van Tieghem himself excepts from the application of the stelar doctrine, namely, the cases of "astely". Already in the earliest paper containing the germ of the stelar idea we find certain cases not covered by the general statement of the existence of a cylinder in the stem. In 1886 these cases together with other similar ones were called astelic, and more recently still the conception has been further elaborated.

The conception is governed throughout by the idea of the endodermis as a definite morphological layer, always separating stelar from extra-stelar tissue. And the endodermis is to be recognised by the suberised thickenings on its radial walls. It is simply by the disposition of layers of cells so thickened that we are supposed to be able to distinguish the various arrangements described. It is easy to show that this criterion is quite illegitimate. The term endodermis is defined by Van Tieghem as the innermost layer of the cortex which "offre fréquemment" the special character in question (10, pp. 738-9). Not only, however, do cell layers with the same character occur in quite other situations (*e.g.*, in the middle of many periderms), but the innermost layer of the cortex certainly does not always possess it. So that these thickenings cannot be used to mark a layer of invariable morphological value. And even in *Equisetum*, Van Tieghem does not keep to his own criterion. For when the "astelic gamodesmic" passes to the apparently monostelic condition we are told that the inner endodermis is still present though its special characters have disappeared. But, we may well ask, if such great importance is to be attached to these special characters as to justify us in founding new types of structure simply upon the disposition of the layers exhibiting them, why should we be suddenly asked to recognise as equivalent a layer which does not exhibit them? The criterion becomes completely chimerical.

Strasburger (15) has pointed out that an endodermoid layer is an air-tight barrier which does not prevent the passage of water through its cells. Such a layer is found in a position to shut off the water-conducting system of a plant from its air-containing lacunar system, but this position may vary within the same genus (*Ranunculus*, *Equisetum*), and has no necessary connection with any morphological region. As a matter of fact it is most often formed from the inner layer of the cortex, but may be developed from conjunctive tissue, or even (leaf of *Isoetes*) from intra-fascicular parenchyma. Since the innermost

layer of the cortex does not always possess the special thickenings which give it the right to be called a "membrane," Strasburger objects to Van Tieghem's re-definition of the word endodermis, and proposes to substitute the term *Phloeoterma*, to be applied to the inner layer of the cortex, *i.e.*, to be used in the strictly morphological sense, whether this inner layer has special characters or not, and to reserve the term endodermis in accordance with its original sense for any sheath or membrane composed of cells with suberised radial walls or other distinctive thickenings, without reference to its position. This revised terminology certainly helps us to get rid of the confusion of thought manifested in Van Tieghem's use of the word endodermis. Strasburger concludes that as all species of *Equisetum* agree in possessing a ring of simple collateral bundles, they should all be considered monostelic, whether the phloeoterma be developed as a general endodermis, or each bundle possess a special endodermis, the phloeoterma having no characters by which it can be distinguished. The same considerations would apply to the genus *Ranunculus* and the other cases of "astely". While we must fully admit the general force of his argument on the ground of comparative anatomy, it is difficult to agree with the following sentence: "Die Grenze der Rinde gegen den Centralcylinder ist dort wo sie sich nicht besonders als Endodermis oder Stärkescheide markirt, nur theoretisch zu ziehen, dieselbe ist aber für alle Fälle festzuhalten" (15, p. 484). How is one to "hold fast" a limit which one cannot distinguish? We can only refer to the remarks which have been already made upon this subject, but we shall briefly recur to the subject in considering the development of the stele.

Leaves furnish us with excellent examples of the frequent impossibility of separating stelar from extra-stelar tissue. Putting aside those cases in which one or more steles from the polystelic stem directly enter the petiole (*Gunnera*, Ferns), we have to consider the ordinary case in a flowering plant, where we have one or more bundles leaving the cylinder and passing into the petiole.

These bundles are accompanied by a certain amount of closely associated parenchyma belonging to the external conjunctive of Flot, a tissue which in the leaf Van Tieghem now calls *peridesm* (16). The bundles are sometimes arranged in a ring, and the whole may be, though comparatively rarely, surrounded by an endodermis. The petiole is then, according to Van Tieghem (10, p. 842), monostelic. In the commoner case where each bundle has an endodermis of its own the petiole is astelic.

Strasburger prefers the term *schizostelic* (15), since the stelar tissue of the petiole represents a separated *portion* or portions of that of the stem. To such a portion he gives the name *schizostele* or *schistostele*[1], at the same time denying the existence of monostelic petioles in Phanerogams on the ground that the apparent pith of the petiole is continuous with the cortex, and not with the pith, of the stem. This last contention brings forward a difficult position. Is it desirable to introduce the question of continuity at all? If we have in the petiole a structure apparently identical with that which we have agreed to call monostelic in the stem, should we be satisfied to call it monostelic here also, without considering the connections of its parts with those of the stem? The strength of Strasburger's position lies in the fact that the continuity, region for region, of the cylinder of root and stem is really the basis of the stelar idea. The origin of the difficulty is to be found in the tendency of a petiole, where it is subject to the same conditions as a stem, to assume the characters of a stem, and among them the arrangement of its vascular tissue according to a radially symmetrical type. We might, perhaps, fitly call such a structure a *pseudostele*.

The mesophyll of the leaf (corresponding with the cortex of the stem) which surrounds the smaller vascular bundles, often has its innermost layer or phloeoterma, which abuts

[1] Van Tieghem has since (17, p. 285) used the word *meristele* for Strasburger's "schizostele," and applied the latter term to the portion of stelar tissue enclosed by each special endodermis in an "astelic" stem. This seems an unwarrantable diversion of the meaning of Strasburger's term.

immediately upon the peridesm of the bundle, specially characterised. The cells of the phloeoterma are often deprived of chlorophyll, or this is confined to the side walls, and these walls may also be suberised. It is, however, a rare case for such layers to be united in a continuous system with the phloeoterma of the stem, and thus to shut off completely, by means of a continuous membrane, the entire stelar system of the plant from its cortical tissue. This state of things obtains, however, in *Pinus* and some dicotyledonous genera, *e.g.*, *Galium*. In most dicotyledonous petioles endodermoid layers, if distinguishable at all, are often incomplete and not necessarily formed from the phloeoterma. A closed sheath to the bundles is, however, often formed in Angiosperm petioles by thickened peridesmic (stelar) tissue, such a sheath being called by Strasburger a *stelolemma* (15). The *ensemble* of the phenomena shows us, clearly enough, that the endodermis, in its original sense, cannot be taken here, any more than in the stem, as a layer of constant morphological value. The phloeoterma may be distinguishable by endodermal or other characters, but on the other hand, it may not.

The main fact in regard to the vascular system of the leaf is one which was pointed out by Van Tieghem in 1870. The system is bilaterally symmetrical in relation to the plane including the organic axes of both leaf and stem, and not, like that of root and stem, radially symmetrical about its organic axis. The designation of the continuous cylinder of root and stem as a *stele* and of each bundle or the whole bundle system of the leaf as a *schistostele* or *meristele* is in complete accord with this general fact. But we must not disguise from ourselves that both the stele and the meristele may not exist in the adult as sharply separated structures.

A. G. Tansley.

(*To be continued.*)

# ON SOME APPLICATIONS OF THE THEORY OF OSMOTIC PRESSURES TO PHYSIOLOGICAL PROBLEMS.

## PART II.

IN my previous article I gave some account of a research by Heidenhain in which this observer, after drawing certain deductions from the theory of osmotic pressures, shows that the phenomena of absorption from the intestinal canal are irreconcilable with these deductions, and are therefore not susceptible of a mechanical explanation, but must be ascribed to the active intervention of cells. Since analogous problems to those discussed by Heidenhain are continually coming before us in physiology, it is important that we should have a clear idea of the factors which are involved in the passage of water or dissolved substances across membranes. I therefore propose to reproduce Heidenhain's statements, and then to consider how far they are true for the special cases which occur in the body.

These statements are as follows :—

1. If two watery solutions with the same osmotic pressure are separated by a membrane through which diffusion can take place, no change in volume occurs on either side of the membrane.

2. If the solutions on either side of the membrane are of unequal osmotic pressure, water passes from the side where the pressure is less to the side where the osmotic pressure is greater.

3. The osmotic pressure of a solution is equal to the sum of the partial pressures of the various dissolved substances.

4. If the solutions on the two sides of the membrane have the same total osmotic pressure but unequal partial pressures of their various constituents, each constituent of the solution passes from the side where it has the higher partial pressure to the other side. No change in the volume of water on the two sides takes place.

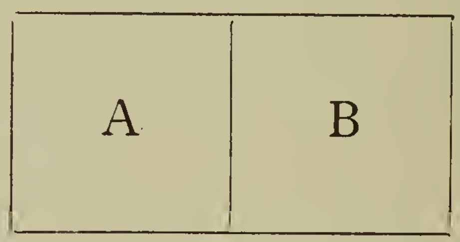
Of these four statements only one (No. 3) is absolutely

correct. The other three are only correct under certain defined conditions which are rarely fulfilled in the body. There are factors at work which have been practically disregarded by most of the recent workers on the subject, and which may tend to produce movement of fluid in apparent opposition to the difference of osmotic pressure. Instances of such cases are afforded in a paper by Lazarus Barlow, to a consideration of whose work we shall shortly return.

There can be no doubt that in the phenomena of transference of fluid or dissolved substances across a membrane the nature of the membrane itself is all-important. I will, therefore, shortly run through the various modes in which interchanges may take place across membranes of varying permeability. We shall see that the close analogy which exists between substances in solution and gases, when dealing with "semi-permeable" membranes, is also borne out by experiment when used to predict the behaviour of solutions separated by such permeable membranes as occur in the body.

The simplest case is that in which two fluids are separated by a perfect semi-permeable membrane that permits the passage of water but is absolutely impermeable to dissolved substances. In this case the transference of water from one side to the other depends entirely on the difference of osmotic pressure between the two sides.

*m.*




If we suppose two vessels, A and B, separated by such a membrane, A containing a solution of $a$ and B a solution of $\beta$, water will pass from A to B so long as the osmotic pressure of $\beta$ is greater than the osmotic pressure of the solution of $a$. If B be subjected to a hydrostatic pressure greater than the osmotic difference between the two fluids, water will pass from B to A until the force causing filtration or transudation (the hydrostatic pressure) is equal to the

force causing absorption into B (the difference of osmotic pressures). Under no circumstance will there be any transference of salt or dissolved substance between the two sides. Such semi-permeable membranes as this, however, rarely occur in the body. It is possible that the external layer of the cell-protoplasm may in some cases resemble the protoplasmic pellicle of plant-cells in possessing this "semi-permeability"; but in nearly all cases where we have a membrane made up of a number of cells, it can be shown that such a membrane permits the free passage of at any rate a large number of dissolved substances.

Let us now consider what will occur when the two solutions A and B are separated by a membrane which permits the free passage of salts and water. If the osmotic pressure of B be higher than A at the commencement of the experiment, the force tending to move water from A to B will be equal to this osmotic difference. But there is at the same time set up a diffusion of the dissolved substances from B to A and from A to B. The result of this diffusion must be that there is no longer a sudden drop of osmotic pressure from B to A, and the result of the primary osmotic difference on the movement of water will be minimised in proportion to the freedom of diffusion which takes place through the membrane. Now let us take a case in which A and B represent equimolecular and isotonic solutions of $\alpha$ and $\beta$. It is evident that the movement of water into A will vary as $Ap - Bp$[1] $= O$. But diffusion also occurs of $\alpha$ into B and of $\beta$ into A. Now the amount of substance diffusing from a solution is proportional to the concentration, and therefore to its osmotic pressure, as well as to its diffusion coefficient.

Hence the amount of $\alpha$ diffusing into B will vary as $Ap \, . \, \alpha k$ (when $k$ is the diffusion coefficient).

In the same way the amount of $\beta$ diffusing into A will vary as $Bp, \beta k'$.

Hence if $\alpha k$ is greater than $\beta k'$, *i.e.*, if $\alpha$ is more diffusible than $\beta$, the initial result must be that a greater number of

[1] $Ap$ = osmotic pressure of A, etc.

molecules of $\alpha$ will pass into B than of $\beta$ into A. Hence the solutions on the two sides of the membrane will be no longer equimolecular, but the total number of molecules of $\alpha + \beta$ in B will be greater than the number of molecules of $\alpha + \beta$ in A, and this difference will be most marked in the layers of fluid nearest the membrane. The result therefore of the unequal diffusion of the two substances is to upset the previous equality of osmotic pressures. The layer of fluid on the B side of the membrane will have an osmotic pressure greater than the layer of fluid in immediate contact with the A side of the membrane, and there will thus be a movement of water from A to B. Hence if we have two equimolecular and isotonic solutions of different substances separated by a membrane permeable to the dissolved substances, there will be an initial movement of fluid towards the side of the less diffusible substance.

We have an exact parallel to this in Graham's familiar experiment in which a porous pot filled with hydrogen is connected by a vertical tube with mercury. In consequence of the more rapid diffusion outwards of the hydrogen than of atmospheric air inwards, the pressure within the pot sinks below that of the surrounding atmosphere, and the mercury rises several inches in the tube. We must therefore conclude that even when the two solutions on either side of the membrane are isotonic, there may be a movement of fluid from one side to the other with a performance of work in the process.

The experimental proof of the truth of this argument is to be found in a recent paper by Dr. Lazarus Barlow. This observer—after pointing out that the huge total osmotic pressures of the salt solutions in the body can very seldom come into play—insists on the fact that the most important point to study in this regard is the initial changes that take place between dissimilar fluids separated by a membrane—as he terms it—the initial rate of osmosis. For this purpose he employs a funnel, the neck of which is prolonged into a capillary tube, while on the mouth is tied a piece of peritoneal membrane. The funnel is filled with the solution whose osmotic attraction for water it is wished

to measure, and its mouth covered with the membrane is immersed in distilled water or in dilute serum.

The experiments which are the most interesting are those in which decinormal solutions of glucose, urea, sodium chloride were compared as to their initial rates of osmosis, the outer fluid being water. He concludes from his experiments that, in the case of prepared peritoneal membrane, the initial rates of osmosis of glucose, sodium chloride and urea in equimolecular solutions do not correspond to the ratio between their final osmotic pressures (as estimated by the depression of freezing-point), but the initial rate of osmosis of glucose (*i.e.*, the rate with which water passes into this solution) is greater than that of sodium chloride, and the initial rate of osmosis of sodium chloride greater than that of urea.

In these experiments the only two solutions which are strictly comparable are those of urea and glucose ($\Delta = 0{\cdot}189°$ C.), since the decinormal Na Cl solution had nearly double the osmotic pressure of these two ($\Delta = 0{\cdot}351$). In three typical experiments, each of which lasted three hours, the average rates at which the fluid in the funnel increased in volume during the first hour were: in the case of glucose, $7\frac{1}{6}$ mm. in five minutes; in the case of sodium chloride, $4\frac{3}{8}$ mm.; and in the case of urea, $1\frac{5}{24}$ mm. These figures are evidently not proportional to the difference of osmotic pressures between the fluid and the funnel and the water in the reservoir. But we have already seen that the moving force is not the total difference of pressure between the fluids in the vessels on either side of the membrane, but the difference of pressure between the layers of fluid in immediate contact with each side of the membrane. The fall of osmotic pressure across the thickness of the membrane varies inversely as the rate of diffusion of the dissolved substance. The question arises therefore whether the results obtained by Lazarus Barlow can be accounted for by differences in the rate of diffusion. In the carefully worked-out tables by this observer we have all the data necessary to decide the question. In the case of glucose, the freezing-point of the solution at the begin-

ning of the experiment was $-0{\cdot}189°$; at the end of the three hours' experiment it was $-0{\cdot}177°$ C.—corresponding to a loss of 6 per cent. of the dextrose. In the case of the urea, the freezing-point at the beginning was $\cdot 189°$, and at the end was $-0{\cdot}154°$ C., a loss of 18 per cent. Here then the initial rate of osmosis of the glucose was about five times that of the urea; the loss by diffusion of the glucose was about one-third that of the urea. In the case of the sodium chloride the loss amounted to 22 per cent.; but here the total difference of osmotic pressure was very nearly double that of the other two solutions, and the result is that the initial rate of osmosis of the sodium chloride takes an intermediate place between that of urea and that of glucose.

In this paper the results of another experiment are given to show that osmosis may occur from a fluid having a higher final osmotic pressure *towards* a fluid having a lower final osmotic pressure. If, for example, equimolecular solutions of sodium chloride and glucose be separated by a peritoneal membrane, the osmotic flow will take place from the fluid having the higher final osmotic pressure—sodium chloride. We might compare with this experiment the results of separating hydrogen at one atmosphere's pressure from oxygen at two atmospheres' pressure by means of a plate of graphite. In this case the initial result will be a still further increase of pressure on the oxygen side of the diaphragm—a movement of gas against pressure taking place in consequence of the greater diffusion velocity of hydrogen.

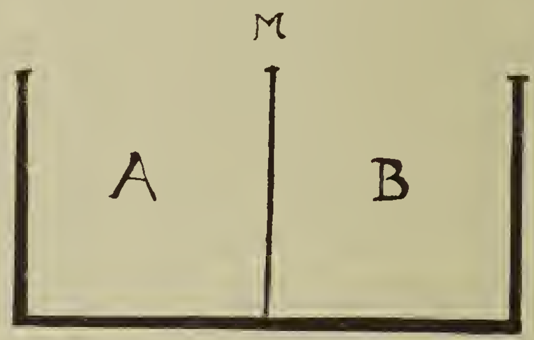
So far we have only considered the behaviour of solutions when separated by a membrane, the permeability of which to salts is comparable to that of water; so that the passage of salts through the membrane depends merely on the diffusion rates of the salts. There can be no doubt, however, that we might get analogous movements of fluid against total osmotic pressure determined, not by the diffusibility of the salts, but by the permeability of the membrane for the salts—a permeability which may depend on a state of solution or attraction existing between membrane

and salts. We have a familiar analogue to such a condition of things in the passage of gases through an india-rubber sheet. If two bottles, one containing carbonic acid, the other hydrogen, be separated by a sheet of india-rubber, $CO_2$ passes into the hydrogen bottle more quickly than hydrogen can pass out into the $CO_2$ bottle, so that a difference of pressure is created between the two bottles, and the rubber bulges into the $CO_2$ bottle. We might, in the same way, conceive of a membrane which permitted the passage of dextrose more easily than that of urea. With such a membrane, experiments conducted in the same way as Dr. Barlow's, would lead to diametrically opposite results. The importance of the membrane in determining the direction of the osmotic passage of fluid is well illustrated by Raoult's experiments. When alcohol and ether were separated by an animal membrane, alcohol passed into the ether, whereas if vulcanite were employed for the diaphragm, the osmotic flow was in the reverse direction, and an enormous pressure was set up on the alcohol side of the diaphragm.

Here we have a possible clue to the "explanation" of many phenomena of cell activity, to which the term "vital" is often assigned. In the swimming-bladder of fishes, for instance, we find a gas which is extremely rich in oxygen, and the oxygen is said to have been secreted by the cells lining the bladder. It is, however, very possible that the processes here may be exactly analogous to Graham's atmolysis, and that the bladder may represent a perfected form of Graham's india-rubber bag.

The next point to be considered is the passage of a dissolved substance across membranes in consequence of differences in the partial pressure of the substance in question on the two sides of the membrane. Great stress is laid by Heidenhain and his pupil Orlow on the fact that in the peritoneal cavity, as well as from the intestine, salt may be taken up from fluids containing a smaller percentage of this substance than does the blood plasma, and they regard this absorption as pointing indubitably to an active intervention of living cells in the process. This argument

requires examination. Supposing the two vessels A and B to be separated by a membrane which offers free passage to water, and a difficult passage to salts. Let A contain ·5




per cent. salt solution and B a solution isotonic with a 1 per cent. Na Cl, but containing only ·65 per cent. of this salt, the rest of its osmotic tension being due to other dissolved substances. If the membrane were absolutely "semi-permeable," water would pass from A to B until the two fluids were isotonic, *i.e.*, until A contained 1 per cent. Na Cl (we may regard volume of B as infinitely great to simplify the argument). If, however, the membrane permitted passage of salt, the course of events might be as follows: At first water would pass out of A, and salt would diffuse in until the percentage of Na Cl in A was equal to that in B. There would now be an equal partial pressure of Na Cl on the two sides of the membrane, but the total osmotic pressure of B would still be higher than A. Water would therefore still continue to pass from A to B more rapidly than the other ingredients of B could pass into A. As soon, however, as more water passed only from A, the percentage of Na Cl in A would be raised above that in B. The extent to which this occurs will depend on the impermeability of the membrane. As soon, however, as the Na Cl in A reaches a certain concentration it will pass over into B, and this will go on until equilibrium is established between A and B. Extending this argument to the conditions obtaining in the living body, we may conclude that neither the raising of the percentage of a salt in any fluid above that of the same salt in the plasma, nor the passage of a salt from a hypotonic fluid into the blood plasma, can afford in itself any proof of an active intervention of cells in the process.

Thus in the case of the pleura we seem to have a membrane which is very imperfectly semi-permeable. It is permeable to salts, but presents rather more resistance to their passage than to the passage of water. Hence on injecting ·5 per cent. Na Cl solution into the pleural cavity water passes from the pleural fluid into the blood, until the percentage of sodium chloride in the fluid is raised perceptibly above that in the blood plasma. The limit of the resistance of the pleural membrane to the passage of salt is, however, soon reached, and then salt passes from pleural fluid into blood; but in every case this passage is from a region of higher to a region of lower partial pressure. Hence at a certain stage of the experiment we find a higher percentage of salt in the pleura than in the blood-vessels, although the total amount of salt in the pleural fluid is less than that originally put in, or, in other words, salt has been absorbed.

We have already seen that the *effective* osmotic pressure of a substance, *i.e.*, its power of attracting water across a membrane, varies inversely as its diffusibility, or as the permeability of the membrane to it. What then will be the effect supposing that on one side of the membrane we place some substance in solution to which the membrane is impermeable?

We will suppose that A and B both contain 1 per cent. Na Cl, but that B contains in addition some substance $x$ to which the membrane is impermeable. Since the osmotic pressure of B is higher, by the partial pressure of $x$, than that of A, fluid will pass from A to B by osmosis. But the consequence of this passage of water will be to concentrate the Na Cl in A, so that the partial pressure of this salt in A is greater than in B. Na Cl will therefore diffuse from A to B with the result that the former difference of total osmotic pressure will be re-established. Hence there will be a continual passage of both water and salt from A to B, until B has absorbed the whole of A. This result will be only delayed if the osmotic pressure of A is at first higher than B, in consequence of a greater concentration of Na Cl in A. There may be at first a flow of fluid

from B to A, but as soon as the Na Cl concentration on the two sides has become the same by diffusion the power of $x$ to attract water from the other side will make itself felt, and this attraction will be proportional to the osmotic pressure of $x$.

We have an example of such a process in the absorption of salt solutions from the connective tissues by the blood-vessels, as well as in the absorption of the normal tissue lymph. The capillaries of the connective tissues of the limbs and peripheral parts of the body are almost impermeable to proteids. In consequence of this impermeability the fluid which is transuded from the capillaries under pressure contains very little proteid, whereas it contains exactly the same proportion of salts as does the blood plasma. It seems probable therefore that the proteid left in solution in the capillaries must exert a certain osmotic attraction on the salt solution outside the capillaries. It is easy to measure this attractive force. If blood serum be placed in a small thistle funnel, on the open end of which is stretched a layer of membrane soaked in gelatine, and the inverted funnel be immersed into salt solution which is isotonic or even hypertonic as compared with the serum, measured by the freezing-point, within the next two to four days fluid will pass into the funnel and rise up in its capillary stem to a considerable height. I have found that the osmotic pressure of the non-diffusible constituents of blood serum measured in this way amounts to between 30 mm. and 40 mm. Hg. Now although this osmotic pressure is so small, it is of an order of magnitude comparable with that of the hydrostatic pressure in the capillaries. This fact is of importance in that, whereas the capillary pressure determines transudation from the vessels, the effective osmotic pressure of the serum (proteids?) determines absorption by the blood-vessels. Moreover the osmotic attraction of the serum for the extravascular fluid will be proportional to the force expended in the production of this extravascular fluid, so that at any given time there must be a balance between the hydrostatic pressure in the capillaries and the production or absorption of fluid from the extravascular

spaces—a balance which is known to obtain under physiological conditions. If we increase the volume of circulating fluid we increase intracapillary pressure and the blood volume tends to diminish in consequence of increased transudation. If we diminish the capillary pressure by bleeding the animal, absorption will predominate over exudation, and the volume of circulating fluid will tend to increase towards its normal amount.

From this cursory study of some of the simplest examples of transference of fluids and salts across membranes, we may draw certain conclusions as to the main factors which are of importance for the process.

These are : (1) The permeability of the membrane to the dissolved substances. This permeability may be of the same character as the permeability of water, in which case the rates of passage of the dissolved substances across the membrane vary as their diffusibilities, and are therefore probably some function of their molecular weights. On the other hand the membrane may exhibit a certain attraction for, or power of dissolving, some dissolved substances to the exclusion of others, in which case there will be no relation between the diffusibilities and rates of passage of the dissolved substances.

(2) The osmotic pressure of the solutions. It is evident that the rules deduced by Heidenhain from the accepted theory of osmotic pressures, and quoted at the beginning of this article, are fallacious in consequence of a too narrow consideration of this second factor to the exclusion of the first. At the same time it must be confessed that our knowledge of the permeability of different membranes to different substances, as well as of the factors on which this permeability depends, is still in an embryonic condition. There can be no doubt that a careful exploration of this field of research would yield results not only interesting to the physicist, but also of incalculable value to the physiologist in his investigation of the phenomena of living things.

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# Science Progress.

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## THE PAST, PRESENT AND FUTURE WATER SUPPLY OF LONDON.[1]

In a discourse to the Members of the Royal Institution on the subject of the Metropolitan Water Supply nearly thirty years ago, I stated that out of every thousand people existing upon this planet at that moment, three lived in London; and, as the population of London has in the meantime doubtless grown at a more rapid rate than that of the rest of the world, it will probably be no exaggeration to say that now, out of every thousand people alive on this earth, four live in London; and therefore any matter which immediately concerns the health and comfort of this vast mass of humanity may well merit our most earnest attention. Amongst such matters that of the supply, in sufficient quantity, of palatable and wholesome water is certainly not the least in importance.

It is not therefore surprising that this subject has received much attention from several Royal Commissions,—notably from the Royal Commission on Water Supply of 1867, presided over by the Duke of Richmond, the Royal Commission on the Pollution of Rivers and Domestic Water Supply of Great Britain, presided over by the late Sir William Dennison, of which I had the honour to be a member; and lastly the Royal Commission, appointed in 1892 to inquire into the Water Supply of the Metropolis,

[1] A discourse delivered at the Royal Institution, 21st February, 1896.

of which Lord Balfour of Burleigh was Chairman, and of which Professor Dewar was a member.

The Royal Institution has also for nearly three-quarters of a century been prominently connected with the investigation and improvement of the Metropolitan Water Supply; no less than four of our Professors of Chemistry having been successively engaged in this work, *viz.*, Professors Brande, Odling, Dewar, and myself, whilst three of them have been members of the Royal Commissions just mentioned. I may therefore perhaps be excused for accepting the invitation of our Secretary to bring the subject under your notice for the third time.

On the present occasion I propose to consider it from three points of view, *viz.*, the past, the present and the future; and, for reasons which will appear hereafter, I shall divide the past from the present at, or about, the year 1883, and will not go back farther than the year 1828, when Dr. Brande, Professor of Chemistry in the Royal Institution; Mr. Telford, the celebrated engineer; and Dr. Roget, Secretary of the Royal Society were appointed a Royal Commission to inquire into the quality and salubrity of the water supplied to the Metropolis.

The Commissioners made careful examinations and analyses, and reported as follows: "We are of opinion that the present state of the supply of water to the Metropolis is susceptible of, and requires, improvement; that many of the complaints respecting the quality of the water are well founded, and that it ought to be derived from other sources than those now resorted to, and guarded by such restrictions as shall at all times ensure its cleanliness and purity. (At this time the water was pumped from the Thames between London Bridge and Battersea.) To obtain an effective supply of clear water free from *insects* and all suspended matter, we have taken into consideration various plans of filtering the river water through beds of sand and other materials; and considering this, on many accounts, as a very important object, we are glad to find that it is perfectly possible to filter the whole supply, and this within such limits, in point of expense, as that no serious objection can

be urged against the plan on that score; and with such rapidity as not to interfere with the regularity of service."

*Before the year 1829*, therefore, the river water supplied to London was not filtered at all; but after the issue of this report, the Companies set themselves earnestly to work to improve the quality of the water by filtration.

The first filter, on a working scale, was constructed and brought into use by the Chelsea Water Company in the year 1829. But even as late as 1850 only three out of the seven principal companies filtered the river water which they delivered in London; and it was not until 1856 that filtration was made compulsory by Act of Parliament, whilst it can scarcely be doubted that, between this date and the year 1868, when my observations on turbidity were first commenced, the operation was very imperfectly performed.

In the year 1832, and again in 1849, London was severely visited by epidemic cholera, and the agency of drinking water in spreading the disease forced itself upon the attention of the observant portion of the medical profession. It was Dr. Snowe, however, who in August, 1849, first formally enunciated the doctrine that drinking water polluted by choleraic matters is the chief mode by which cholera is propagated.

Received at first with incredulity, this doctrine was supported by numerous facts, and it soon caused renewed attention to be directed to the quality of the water then being supplied to the Metropolis; with the result that the intakes of the various Companies drawing from rivers were, one after another, removed to positions above the reach of tidal influence; the Thames water being withdrawn from the river above Teddington Lock, and the Lea water at Ponder's End, above the tidal reaches of that river.

In every visitation of Asiatic cholera to London, the water supply was either altogether unfiltered or imperfectly filtered, besides being derived from highly polluted parts of the Thames and Lea; and the enormous loss of life, amounting in the aggregate to nearly 36,000 people, can only be attributed to this cause. It has been abundantly proved that efficient filtration is a perfect safeguard against

the propagation of the disease, and since the year 1854 no case of Asiatic cholera in London has been traced to the use of *filtered* river water.

These are the results arrived at by the most general investigation of the subject. They show that in every epidemic, the mortality varied directly with the intensity of the drainage pollution of the water drunk by the people; but if time permitted, a more detailed study of the statistics in both epidemics would demonstrate, much more conclusively, this connection between cholera mortality and the pollution of drinking water—a connection which has quite recently been terribly emphasised in the case of Hamburg.

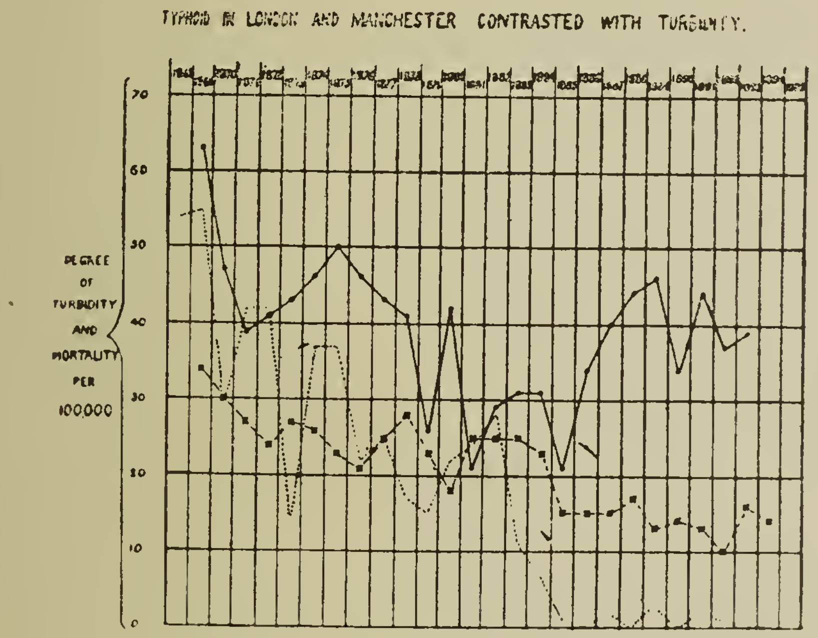
Such is the verdict with regard to *cholera*, and the same is true of that other great water-borne disease *typhoid fever*. But, unlike cholera, this disease is disseminated in several other ways, and its presence or absence in any locality may not, of necessity, have any connection with drinking water, as is strikingly shown by the health statistics of Manchester.

There is no evidence whatever that, since the year 1869, when typhoid fever appeared for the first time as a separate disease in the Registrar General's reports, it has been conveyed by the water supply of the Metropolis. An inspection of the diagram (No. 1) shows, it is true, a greater proportional mortality during the period of imperfect filtration than during the later period; that is to say from 1883 when the process began to be performed with uniform efficiency; but the plotting of a similar curve for the deaths by typhoid in Manchester shows that this disease arises from other causes than polluted water, since the water supply of Manchester, derived as it is from mountain sources, is above all suspicion of this kind. These other causes have during the last ten years been much mitigated in London by various sanitary improvements; whilst, as shown in the diagram, there has been no corresponding mitigation in Manchester.

Although very soon after the year 1856 all the water supplied to the Metropolis was obtained from sources much less exposed to drainage pollution, it was still very carelessly

filtered. Previous to the year 1868, there are no records of the efficiency, or otherwise, of the filtration of the Metropolitan water supply derived from rivers, as distinguished from deep wells, the water of which is perfectly clear without filtration.

It was in the year 1868 that I first began to examine the water supplied to the Metropolis from rivers *with reference to efficiency of filtration.* In that year, out of eighty-four samples examined, seven were very turbid, eight turbid, and ten slightly turbid, so that altogether no less than nearly 30 per cent. of the samples were those of *inefficiently* filtered water. The Metropolitan Water Supply then, up to the year 1868, may be shortly described as




No. 1.

derived for many years from very impure sources with either no filtration at all, or with very inefficient filtration; and afterwards, when the very impure sources were abandoned, the supply was still often delivered in a very inefficiently filtered condition. But, after the establishment of monthly reports on the filtration of the river-derived supplies, the quality of these waters gradually improved in this most important respect, as is seen from diagram No. 1. In this diagram, the continuous line with dots represents the mortality from typhoid in Manchester, the broken and eroped line the contemporaneous mortality in London, and the dotted curve the degree of turbidity of the London water supply.

These observations graphically represented in the diagram show that, at the time they were commenced, the filtering operations were carried on with considerable carelessness, and that this continued, though to a less extent, down to the year 1883, since which time, and especially since 1884, the efficiency of filtration of all the river waters supplied to the Metropolis has left little to be desired.

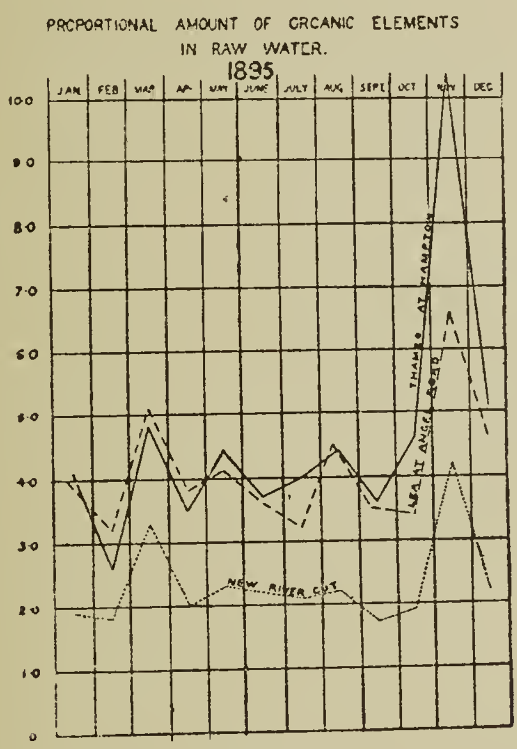
What is it then that separates the *past* from the *present* water supply of London? In the first place there is the change of source—I mean the change in position of the intakes of the several Companies drawing from the Thames and Lea—and the total abandonment of the much-polluted river Ravensbourne by the Kent Water Company. So long as the water supply was derived from the tidal reaches of the Thames and Lea, receiving as these reaches did the drainage of immense populations, the risk of infection from water-borne pathogenic organisms could scarcely be otherwise than imminent; for, although we now know efficient filtration to be a perfect safeguard, anything short of efficiency must be attended with risk in the presence of such extreme pollution.

Nevertheless, the line of demarcation between the past and the present water supply of the Metropolis is, in my opinion, to be drawn, not when the intakes of the river companies were removed to positions beyond the possibility of pollution by the drainage of London, but it must be drawn at the time when *efficient filtration* was finally secured and ever since maintained, that is to say, in the year 1884.

The removal of turbidity by sand filtration, however, refers only to suspended matters; but there are sometimes objectionable substances in solution of which organic matter is the most important. River water and mountain water, even when efficiently filtrated, contains more organic matter than spring or deep well water; but this is reduced in quantity by storage and especially by filtration, although these waters can perhaps never be brought up to the standard of organic purity of spring and deep well water.

## THE PRESENT WATER SUPPLY.

At present London is supplied with water from four sources—the Thames, the Lea, the New River, and deep wells. Of these the deep wells yield as a rule the purest water, requiring no filtration or treatment of any kind before delivery for domestic use. The river waters, on the other hand, require some kind of treatment before delivery—storage, subsidence in reservoirs, and filtration. The water from the Thames is abstracted at and beyond Hampton, far above the reach of the tide and London drainage. The water from the Lea is taken out at two points, *viz.*, at Angel Road near Chingford, by the East London Water Company,

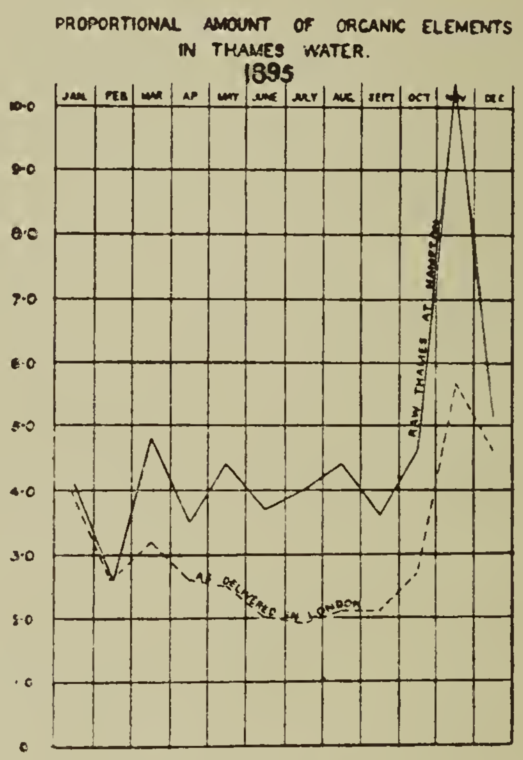



No. 2.

and above Hertford by the New River Company, who convey it to Green Lanes by an open conduit twenty-five miles long, called the New River Cut, in which it is mixed with a considerable volume of spring and deep well water.

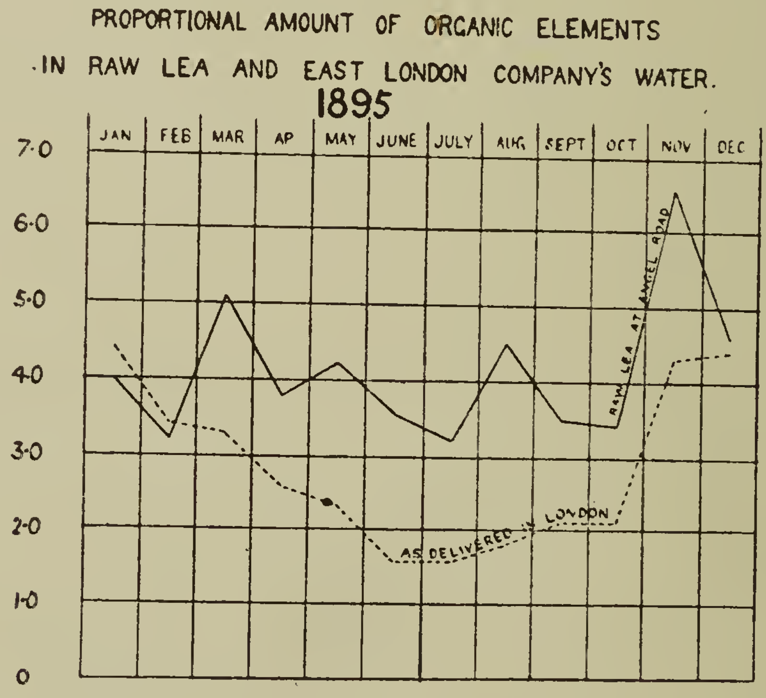
All three river waters are affected by floods and are, as raw materials, of considerably different quality as regards organic purity (see diagram No. 2). From these raw materials by far the largest volume of the Metropolitan Water Supply is derived, and the chemical or organic purity of the water sent out to consumers stands in direct relation to the organic purity of the raw material used, as

is seen from the diagrams Nos. 3, 4 and 5, which show the proportional amounts of organic elements in the raw and filtered waters; they also show the advantage of storage in excluding flood water, No. 4 shows that floods in March




No. 3.

and August were circumvented, but not in November. The numbers in the margins of the diagrams express the proportional amount of organic elements, that in the Kent



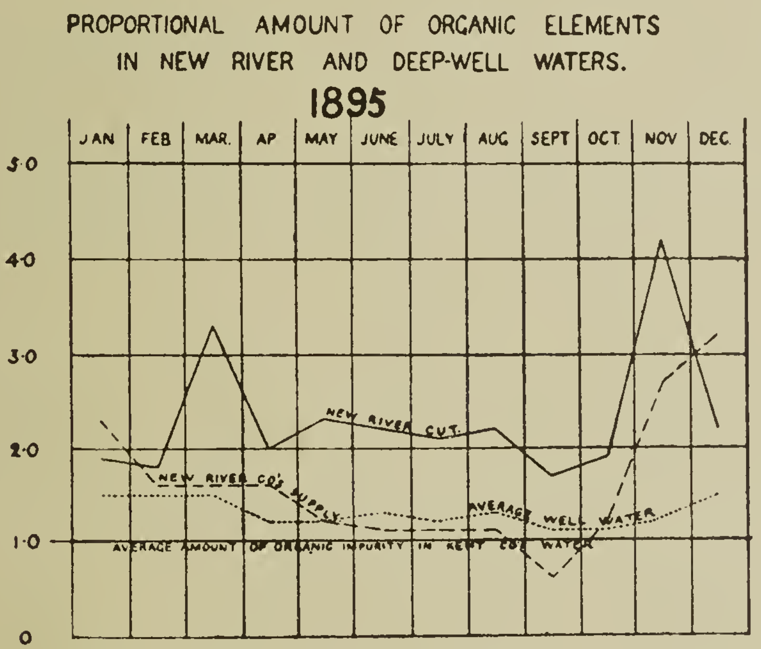

No. 4.

Company's water during the nine years ending December 1876 being taken as unity, as is depicted in diagram No. 5.

Hitherto I have spoken of *chemical* purity or comparative freedom from organic matter only, but the spread of diseases

such as cholera and typhoid fever through the agency of drinking water has no connection whatever with the chemical or organic purity of the water. These diseases are propagated by living organisms of extreme minuteness, to which the names bacilli, bacteria, and microbes have been given, and here comes the important question how, if at all, does filtration secure immunity from these water-borne diseases?

To Dr. Koch of Berlin, we are indebted for the answer to this question. By his discovery of a means of isolating and counting the number of bacteria, or bacilli, or microbes and their spores in a given volume of water, we were, for the first time, put into possession of a method by which the condition of water as regards these living organisms, before




No. 5.

and after filtration, can be determined with quantitative exactness. The enormous importance of this invention (which was first made known and practised in England in 1882 by the late Dr. Angus Smith) is evident, when it is borne in mind that the living organisms, harmful or harmless, contained in water are of such extreme minuteness as practically to defy detection by ordinary microscopical examination. But although the microscope cannot detect with certainty single bacteria or their spores, even the naked eye can easily discern *towns* or *colonies* consisting of thousands or even millions of such inhabitants.

Dr. Koch's method accomplishes at once two things: it isolates, in the first place, each individual microbe or

germ; and, secondly, places it in conditions favourable for its multiplication which takes place with such amazing rapidity that, even in a few hours, or at most in two or three days, each organism will have created around itself a visible colony of innumerable members—a town in fact comparable to London itself for population.

By operating upon a known volume of water, such as a cubic centimetre for instance, the number of separate organisms or their spores, in a given volume of the water under investigation can thus be determined. The following is the method now adopted in carrying out Koch's process for the bacterial investigation of drinking water:—

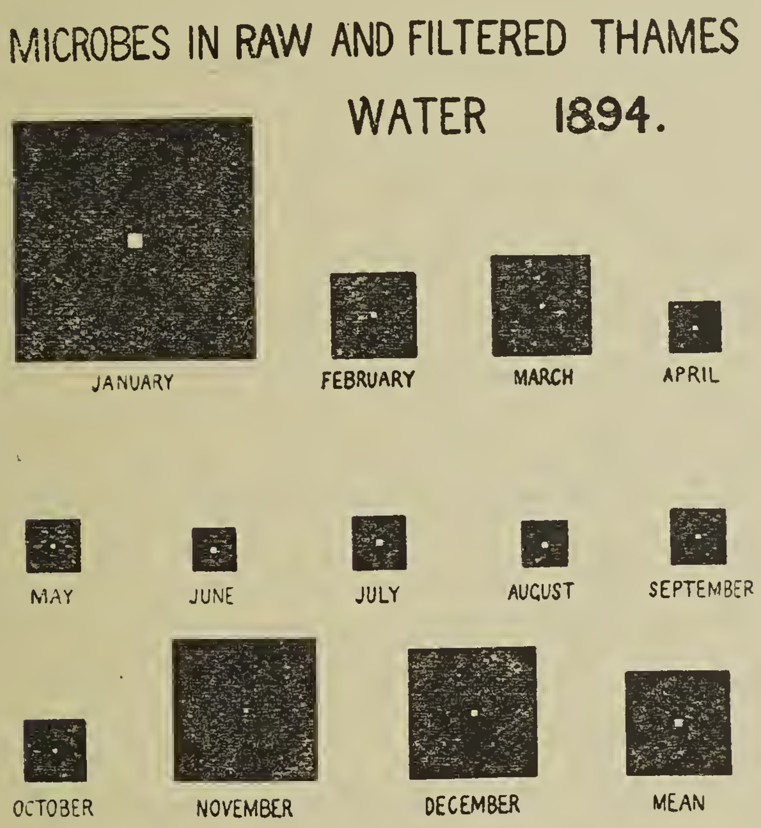
1. Preparation of the nutritive medium.
2. Sterilisation of the medium.
3. Collection of the sample of water in a vacuous tube afterwards to be hermetically sealed.
4. Transport of the sample to the bacteriological laboratory, *packed in ice* to prevent multiplication.
5. Mixture of a known volume of the water sample with the nutrient medium.
6. Casting of the mixture into a solid plate.
7. Incubation of the solid plate.
8. Counting of the colonies.
9. Examination of separate colonies, or rather of the individual members under the microscope.

Sometimes the cultivations are made upon a plate of the substance called *agar* which resembles isinglass, and bears a temperature of blood heat without melting.

In order to ascertain the effect of filtration upon the bacterial quality of water, it is absolutely necessary that the sample should be taken immediately after it has passed through the filters; for, if it be obtained from the delivery mains in town, that is to say, after the water has passed through many miles of pipes, the rapid multiplication of these organisms, except in very cold weather is such, that a water which contains only a single living organism per cubic centimetre, as it issues from the filter, may contain

100 or 1000 in the same volume when, after several hours, it arrives on the consumer's premises.

Now what is the effect of sand filtration as carried out by the various Water Companies supplying London upon the living matter contained in the raw river water? *It is simply astounding:* water containing thousands of bacteria per cubic centimetre, for a single drop of Thames water sometimes contains nearly 3000 separate living organisms, comes out from the sand filters with fifty, thirty, ten, or even less of these organisms per cubic centimetre, or the number of microbes in a single drop is reduced to two or even to zero.




No. 6.

Rather less than one-tenth of the total volume of water supplied to London is derived by the Kent Water Company from deep wells in the chalk. As it issues from the porous rock into the fissures and headings of these wells, this water is, in all probability, absolutely sterile; but by the time it has been pumped up to the surface it usually contains a certain number, though small, of microbes. Thus, during the year 1892 it contained on the average six per cubic centimetre in 1893, thirteen; in 1894, fifteen; and in 1895, eight.

The diagram No. 6 shows graphically the bacterial improvement of the Thames water by filtration during

the year 1894. In this diagram the black squares represent the number of microbes in a given volume of the raw water in each month, and the white centres the number remaining in the same volume after filtration.

Although deep well water has, from a bacterial point of view, a decided advantage, the filtered river waters are not very far behind, and there is every reason to believe that with the improvements which are now being carried out by the various river Water Companies, the Kent Company's deep well water will, before long, be run very hard by the other supplies.

By the examination of the water as it issues from the filters, the utmost freedom from microbes, or maximum degree of sterility of each sample is determined. This utmost freedom from bacterial life after all sources of contamination have been passed is obviously the most important moment in the history of the water; for the smaller the number of microbes found in a given volume at that moment the less is the probability of pathogenic or harmful organisms being present; and although the non-pathogenic may afterwards multiply indefinitely this is of no consequence in the primary absence of the pathogenic; but it is only fair, in describing the character of the present water supply of London, to say that not a single pathogenic organism has ever been discovered even in the *un*filtered water as it enters the intakes of the various Companies, although these organisms have been carefully sought for. It is sometimes said that the non-pathogenic organisms found in water *may* be beneficial to man; but this idea is not borne out by the fact of their entire absence from the food which nature provides for young animals. Healthy milk is absolutely sterile.

As it is at present impracticable to obtain water, uniformly at least, free from microbes, it is desirable to adopt some standard of bacterial purity; and 100 microbes per cubic centimetre has been fixed upon by Dr. Koch and myself as the maximum number allowable in potable water. This standard is very rarely infringed by the London Water Companies, whilst I have every reason to hope that,

in the near future, now that special attention is directed to bacterial filtration, it will not be approached within 50 per cent. This hope is based not only upon my own observations, but also upon the exhaustive and exceedingly important investigations carried out at the Lawrence Experiment Station by the State Board of Health of Massachussetts, under the direction of Mr. George W. Fuller, the official biologist to the Board.

More than six years have already been spent in the prosecution of these American experiments, and many thousands of samples of water have been submitted to bacterial cultivation. The Massachussetts experimental filters are worked at rates up to 3,000,000 gallons per acre daily, which renders the results available for application to public water supplies; indeed none of the water delivered in London is filtered at so rapid a rate as this. It was found that at these rates all the disease-producing germs, which were intentionally and in large numbers added to the unfiltered water, were substantially removed. The filters were so constructed and arranged as to allow direct comparison of the bacterial purification of water under different rates of filtration, with sand of different degrees of fineness, with different depths of the same sand, and with intermittent and continuous filtration.

The actual efficiency of these filters was also tested by the application of the bacillus of typhoid fever. Very large numbers of these bacilli and of other species were applied in single doses to the several filters at different times, and the effluent was examined four times daily for several days afterwards. The results so obtained give a thoroughly trustworthy test of the degree of bacterial purification effected by each of the experimental filters, and these are the data which have been largely used by the Massachussetts State Board of Health in deducing the rules which they consider ought to be observed in water filtration.

Among the subjects investigated by means of these experimental filters were:—

1. The effect, upon bacterial purification, of the rate of filtration.

2. The effect of size of sand grains upon bacterial purification.
3. The effect of depth of material upon bacterial purification.
4. The effect of scraping the filters upon bacterial purification.

Time does not permit of my giving the answers to these questions in detail; but they may be summarised as follows:—

1. The rate of filtration between 500,000 and 3,000,000 gallons per acre per day exercises practically no effect on the bacterial purity of the filtered water. It is worthy of note that the rates of filtration practised by the several Water Companies drawing their supplies from the Thames and Lea are as follows: Chelsea Company, 1,830,000; West Middlesex, 1,359,072; Southwark Company, 1,568,160; Grand Junction Company, 1,986,336; Lambeth Company, 1,477,688; New River Company, 1,881,792; and East London Company, 1,393,920. Hence not one of the London Companies filters at the rate of 2,000,000 gallons per acre per day; at which rate in the Massachussett's filters 99·9 per cent. of the microbes present in the raw water were removed.

2. The effect of size of sand grains was found to be very considerable; and, in confirmation, I find that by the use of a finer sand than that employed by the Chelsea Company, the West Middlesex Company is able, with much less storage, to attain an equal degree of bacterial efficiency.

3. The depth of sand, between the limits of one and five feet, exercises no practical effect on bacterial purity when the rate of filtration is kept within the limits just specified. And this result is quite borne out by my own experience gained in the bacterioscopic examination of the filtered waters of the seven Companies supplying the Metropolis from rivers. Thus the New River Company, with 1·8 feet of sand on the filters, compares favourably with the Chelsea Company, the sand on whose filters is more than twice that depth.

Placed in the order of thickness of sand on their filters, the Metropolitan Companies range as follows: Chelsea,

Lambeth, West Middlesex, Southwark, East London, Grand Junction, and New River. Placed in the order of efficient filtration they range as follows: Chelsea and West Middlesex equal, New River, Lambeth, East London, Southwark, and Grand Junction.

4. When there is such an accumulation of deposit on the surface of a sand filter that, for practical purposes, sufficient water cannot be made to pass through it, the surface of the filter has to be scraped; that is to say, the mud and about half an inch of the sand are removed from the surface. After this operation, there is sometimes an increase in the number of bacteria in the filtered water, and it was noticed that the increase was greater in shallow than in deep filters and with high than with low rates of filtration; and there is no doubt that the effect of scraping is considerably magnified when coarser descriptions of sand are employed, as in the case of the filters of the London Water Companies. I should like, therefore, to impress upon the engineers of these Companies the desirability of using finer sands than are at present employed.

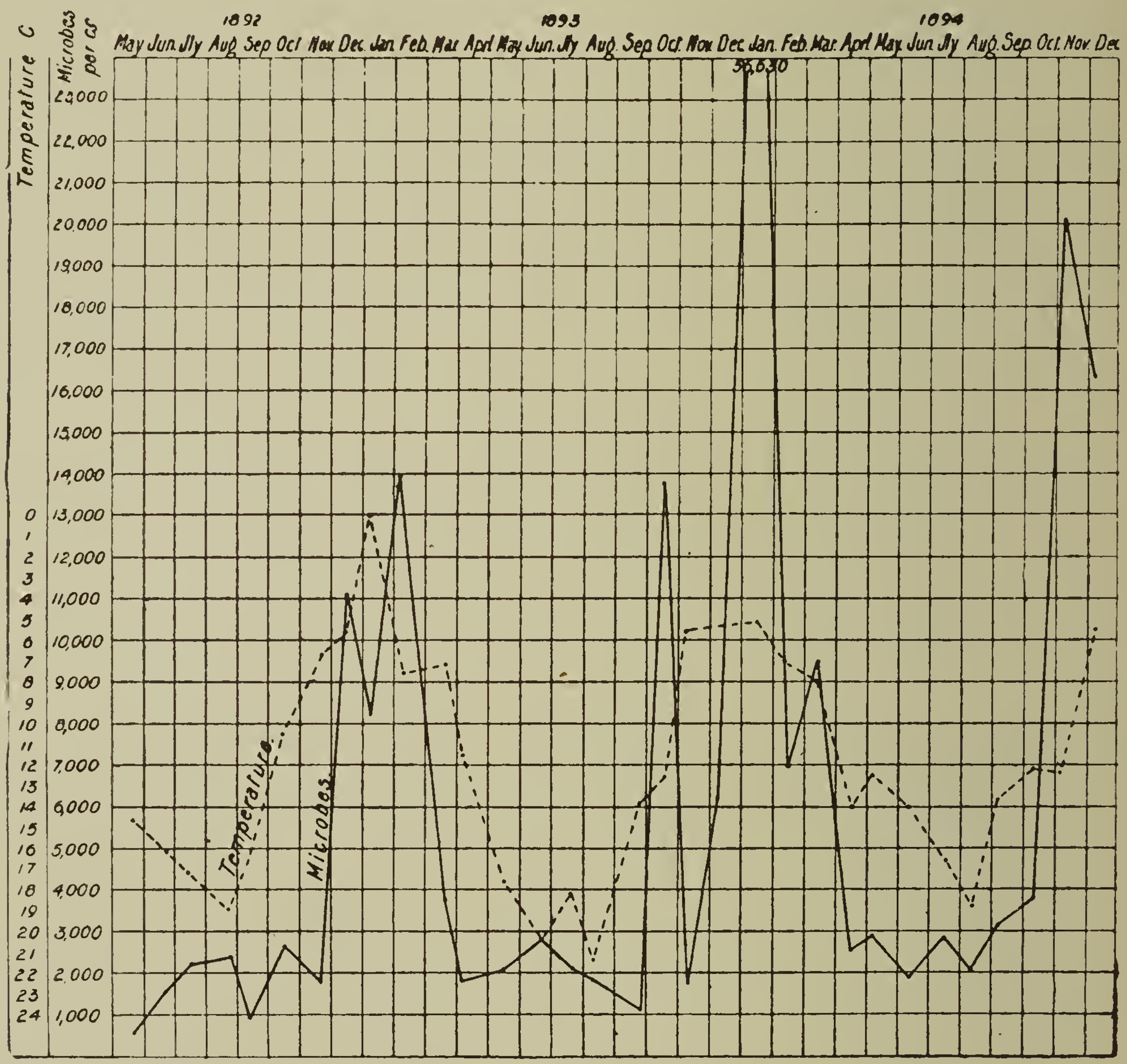
## INFLUENCE OF THE BACTERIAL CONDITION OF THE RAW RIVER WATER UPON THAT OF THE FILTERED EFFLUENT.

I have found that the number of bacteria in a given volume of filtered water is to a considerable extent influenced by the number contained in the raw water supplying the filter; and from this point of view, therefore, the bacterial condition of the raw river water used in the Metropolis is of no inconsiderable importance.

Since May, 1892, I have made monthly determinations of the number of microbes capable of developing on a gelatine plate in a given volume of raw Thames water collected at the intakes of the Metropolitan Water Companies at Hampton; and the number has varied during this time between 631 and 56,630 per cubic centimetre, the highest numbers having, as a rule, been found in winter or when

the temperature of the water was low, and the lowest in summer or when the temperature was high.

Now, besides temperature, there are two other conditions to either of which this difference may be attributed, *viz.*, sunshine and rainfall, and I have endeavoured by a series of graphic representations to disentangle these possible influences from each other by placing the results of the microbe determinations in juxtaposition with (1) the temperature of the water at the time the samples were taken;

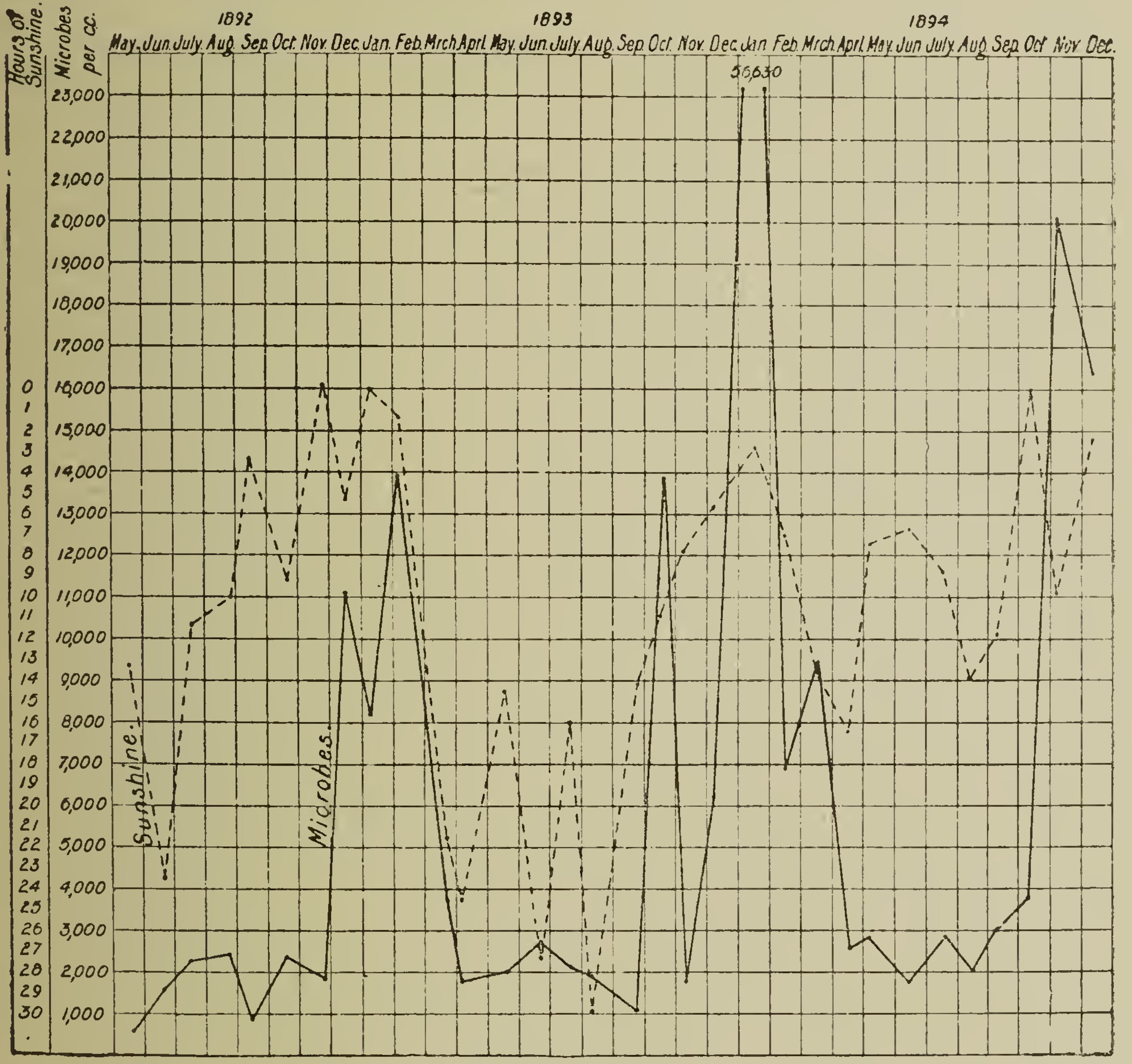



No. 7.

(2) the number of hours of sunshine on the day and up to the hour when each sample was drawn and on the two preceding days, and (3) the flow of the Thames over Teddington Weir on the same day expressed in millions of gallons per twenty-four hours. Although the graphic representations were confined to the Thames, the conditions affecting bacterial life in this river are doubtless equally potent in other rivers and streams.

The samples for microbe cultivation were collected at

about nine inches below the surface of the water in partially exhausted and sealed tubes, the ends of which, when the tubes were lowered to the required depth, were broken off by an ingenious contrivance devised by my Assistant, Mr. Burgess. On being withdrawn from the river the tubes were immediately hermetically sealed and packed in ice for conveyance to my laboratory, where the cultivation was always commenced within four hours of the time of collection.



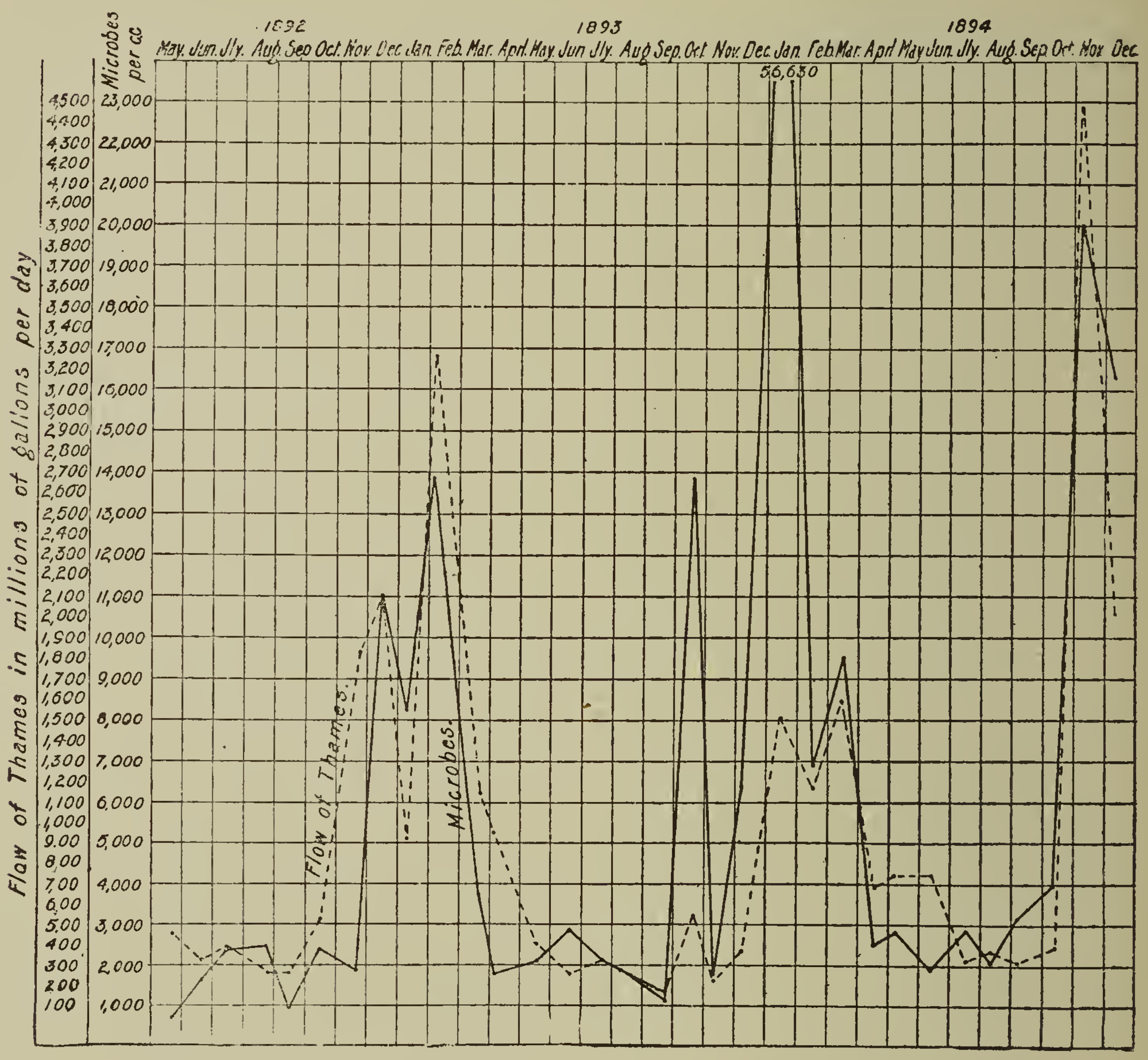

No. 8.

For the records of sunshine, I am indebted to the kindness of Mr. James B. Jordan of Staines; and for gaugings of the Thames at Teddington Weir to Mr. C. J. More, the engineer to the Thames Conservancy Board.

The graphic representation of these collateral observations affords definite evidence as to which of the three conditions—temperature, sunshine, and flow of the river—has the predominant influence upon bacterial life in the water. The first diagram (No. 7) compares the number of

microbes per cubic centimetre with the temperature at the time the sample was taken. The horizontal lines express the numbers of microbes and the temperature, while the vertical lines denote the months when the samples were taken. For obvious reasons the horizontal lines expressing the numbers of microbes and temperatures are numbered in opposite directions.

The diagram shows that although coincidences between a high number of microbes and a low temperature are not




No. 9.

wanting, some other condition entirely masks the effect, if any, of temperature.

The next diagram (No. 8) institutes the comparison between the number of microbes and the hours of sunshine to which the water has been exposed. The diagram is constructed on the same lines as the first.

It is here seen that, as in the case of temperature, there is some other condition which entirely overbears the influence of sunlight in the destruction of microbes in the

river water. This condition is the amount of rainfall higher up the river, or, in other words, the volume of water flowing along the river bed, as is seen from the comparison represented in the next diagram (No. 9).

This diagram shows very conclusively that the volume of water flowing in the Thames is the paramount influence determining the number of microbes. It compares the volume of water in the river gauged at Teddington Weir with the number of microbes found in the raw Thames water at Hampton on the same day. In this diagram, the numbers representing the flow of the river in millions of gallons per day and the number of microbes per cubic centimetre in the water both run from the bottom of the diagram upwards.

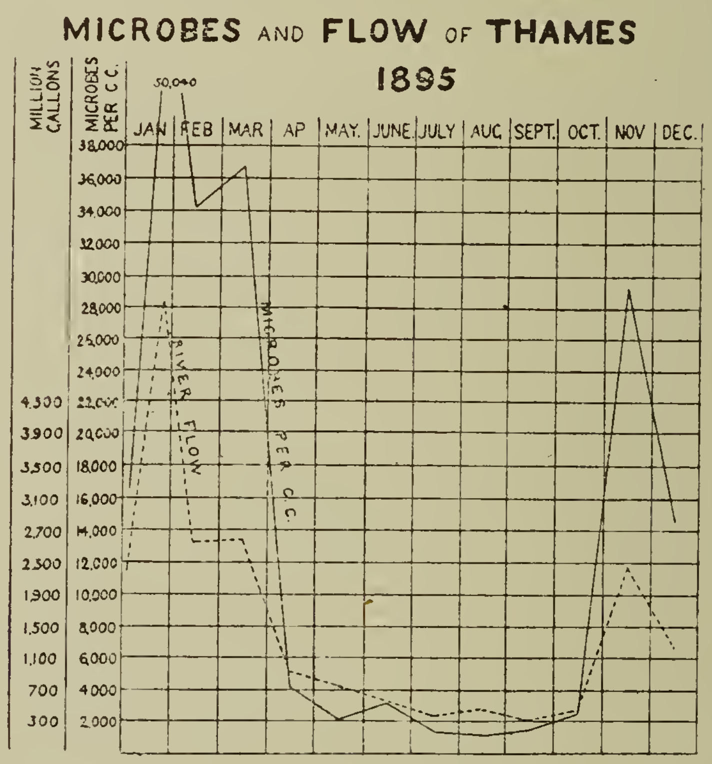
Comparing the curves in the diagram it is seen that, with very few exceptions, a remarkably close relation is maintained between them.

The only exception of any importance to the rule that the number of microbes varies directly with the flow of the river, occurring during the thirty-two months through which these observations were continued, happened in November, 1892, when the flow increased from 501 millions of gallons in October to 1845 millions in November, whilst the microbes actually diminished in number from 2216 to 1868 per cubic centimetre. Neither the sunshine nor the temperature records of these two months, however, afford any explanation of this anomalous result, for there was a good deal of sunshine in October before the collection of the sample and the temperature was higher, whilst in November no ray of sunshine reached the Thames during the three days preceding the taking of the sample and the temperature was nearly 4° C. lower than in the preceding month. I have ascertained, however, that the Thames basin had been twice very thoroughly washed out by heavy floods before the time when the November sample was taken, and this affords a satisfactory explanation of the anomalous result yielded by this sample.

These comparisons demonstrate that the number of microbes in Thames water depends directly upon the rate

of flow of the river, or, in other words, on the rainfall, and but slightly, if at all, upon either the presence or absence of sunshine or a high or low temperature; and they are confirmed by the continuation of these observations during the year 1895 exhibits in diagram No. 10.

With regard to the effect of sunshine upon bacterial life, the interesting observations of Dr. Marshall Ward leave no doubt that sunlight is a powerful germicide; still it is obvious that its potency in this respect must be greatly diminished, if not entirely annulled, when the solar rays have passed through a stratum of water of even com-




No. 10.

paratively small thickness before they reach the living organisms. By a series of ingeniously contrived experiments, Mr. Burgess has demonstrated the correctness of this view.

A sterile bottle about half filled with Thames water was violently agitated for five minutes to insure equal distribution of the organisms. Immediately afterwards a number of sterile glass tubes were partially filled with this water and sealed hermetically. Three of these tubes were immediately packed in ice, and the remainder were attached in duplicate at definite distances apart to a light wire frame which was then suspended vertically in the river. The

experiments were made near the Grand Junction Company's Intake at a place favourable for the sun's rays to fall on the river without any obstruction.

The river was at the time in a very clear condition and contained but little suspended matter ; whilst the day was fine, although clouds obscured the sun occasionally. The tubes were exposed to light in the river for four and a half hours—from 10·30 A.M. to 3 P.M. on 15th May, 1895. At the end of this time the tubes were packed in ice for transport to my laboratory, where the cultivation was started immediately. The colonies were counted on the fourth day and yielded the results given in the table :—

| | No. of Colonies per c.c. |
|---|---|
| Thames water packed in ice immediately after collection | 2127 |
| Thames water after exposure to sunlight for 4½ hours at surface of river | 1140 |
| Thames water after exposure to sunlight for 4½ hours at 6 in. below surface of river | 1940 |
| Thames water after exposure to sunlight for 4½ hours at 1 ft. below surface of river | 2150 |
| Thames water after exposure to sunlight for 4½ hours at 2 ft. below surface of river | 2430 |
| Thames water after exposure to sunlight for 4½ hours at 3 ft. below surface of river | 2440 |

These experiments show that, on 15th May the germicidal effect of sunlight on Thames microbes was nil at depths of one foot and upwards from the surface of the water. It cannot, therefore, excite surprise, that the effect of sunshine upon bacterial life in the great mass of Thames water should be nearly, if not quite, imperceptible. It is thus ascertained that sunlight can only kill the germs, or microbes, near the surface of the water, whilst those at any depth, for the most part, escape destruction.

On the other hand the enormous effect of floods in augmenting the number of microbes can hardly surprise us, for when a great body of water has flowed over the banks of the river, which are at other times dry and exposed, it carries along with it countless impurities—an effect common both to the main stream and its tributaries. The Thames

basin is as it were, on every such occasion, thoroughly washed out, and it is only to be expected that the number of microbes in the water should be enormously increased as is found to be the case.

Now with respect to

THE WATER SUPPLY OF THE FUTURE.

In view of the rapid increase of the population of London, fears have from time to time been entertained that the water supply from the Thames basin, that is to say from the rivers Thames and Lea, supplemented by water from springs and deep wells within the basin itself, would soon be insufficient in quantity; whilst the quality of the water taken from the river has, up to a comparatively recent date, been considered unsatisfactory. On these grounds various schemes have, from time to time, been brought forward for the supply of the Metropolis from other river basins—from the Wye, the Severn, the river basins of North Wales, and of the Lake Districts of Cumberland and Westmoreland.

It is worthy of note, however, that all the Royal Commissions have arrived *unanimously* at the conclusion that the *quantity* of water obtainable from the Thames basin is so ample as to render the necessity of going elsewhere a very remote contingency.

I shall now endeavour to put, very shortly before you a few facts which, in my opinion, prove that, both as regards quantity and quality, the Thames basin will for a very long time to come afford an abundant supply for the Metropolis. There is indeed no river basin in Great Britain which affords such an abundant supply of excellent water as that available in the Thames basin.

Besides that which flows directly into the rivers, this water is contained in the Chalk, Oolite, and Lower Greensand, which are the best water bearing strata in the kingdom. From these rocks it issues in copious springs of unsurpassed organic purity. I have personally inspected every spring of importance in the Thames basin and have

analysed samples of the water. The results, in a very condensed form, are recorded in the annexed Table. Twenty-

SPRING AND DEEP-WELL WATERS IN THE THAMES BASIN.

| Results of Analysis in Parts per 100,000. | Oolite. Average of 21 Samples. | Lower Greensand. Average of 5 Samples. | Chalk. | |
|---|---|---|---|---|
| | | | Springs. Average of 8 Samples. | Wells. Average of 36 Samples. |
| Total Saline Matters - | 27·34 | 18·25 | 30·14 | 37·45 |
| Organic Carbon - - | ·035 | ·032 | ·041 | ·052 |
| Organic Nitrogen - - | ·012 | ·006 | ·010 | ·019 |
| Hardness before boiling | 22·5 | 10·5 | 25·3 | 28·0 |
| ,, after ,, | 5·5 | 3·6 | 4·9 | 6·5 |

one samples of Oolitic spring water were analysed, and every one of these was of even greater organic purity than the water delivered by the Kent Company, which I have always regarded as the standard of organic purity to be aimed at in all other Water Works.

Five springs issuing from the Lower Greensand were examined; and again, every one of these was of even greater purity, organically, than the Kent Company's water; whilst they were, on the average, only one-third as hard. Forty-six samples of water from the Chalk were chemically examined, and these also contained but the merest traces of organic matter.

All these samples from the Chalk were derived from sources where the water-bearing stratum is free from a covering of London clay; but, as soon as the Chalk dips beneath the London Tertiary Sands and clay, the quality of the water undergoes a remarkable alteration. The total solids in solution are greatly increased in amount, whilst the hardness is much mitigated, owing to the replacement of bicarbonate of lime by bicarbonate of soda. These waters are also of high organic purity; but, as the quantity is very limited, it is useless to dwell upon them. They

supply the Trafalgar Square fountains and the London breweries, and we can well afford to leave them to be converted into beer. For dietetic purposes there is no better water in the kingdom than the underground water of the Thames basin. For *sentimental* reasons I should like to see it conveyed to the works of the various companies in special conduits; but we have seen that, on *hygienic* grounds, it may safely be allowed to flow down the bed of the Thames if it be afterwards efficiently filtered.

So much for *quality*, now as to *quantity*; the basins of the Thames and Lea include an area of upwards of 5000 square miles. Of this rather more than one half (including the Oolitic, Cretaceous, and portions of the Tertiary Formations) is covered by a porous soil upon a permeable water bearing stratum. The remainder is occupied by the Oxford, Kimmeridge, Gault, and London Clays; being thus covered by a clay soil upon a stiff and impervious subsoil.

The annual rainfall of the district is estimated at an average of twenty-eight inches. The rivulets and streams of the Thames basin are formed and pursue their course on clay land. There are no streams on the Chalk. That which falls upon this porous stratum and does not evaporate sinks, mostly where it alights, and heaps itself up in the water-bearing stratum below, until the latter can hold no more. The water then escapes as springs at the lowest available points.

Innumerable examples of these springs occur all round the edge of the Thames basin, and at various points within it. Thus from the Chalk they are ejected at the lip of the Gault; and in the Oolitic area by the Fuller's Earth below it, or by the Oxford Clay, geologically, above it.

According to the guagings of the engineer of the Thames Conservancy Board there passed over Teddington Weir, in 1892, 387,000 millions of gallons, equal to an average flow of 1060 millions of gallons daily. In the following year, 1893, their passed over Teddington Weir an aggregate of 324,227 millions of gallons, or a daily average of 888 millions, the average for the two years being

974 millions of gallons, and this number does not include the 120 millions daily abstracted by the five London Water Companies who draw their supplies from the Thames.

Thus, in round numbers, we may say that after the present wants of London have been supplied from this river, there is a daily average of nearly 1000 millions of gallons to spare. Surely it is not too violent an assumption to make that the enterprising engineers of this country can find the means of abstracting and storing for the necessary time one-fourth of this volume.

As regards the quality of this stored water, all my examinations, of the effect of storage upon the chemical and especially upon the bacterial quality, point to the conclusion that it would be excellent. Indeed the bacterial improvement of river water by storage for even a few days is beyond all expectation. Thus the storage of Thames water by the Chelsea Company for only thirteen days reduces the number of microbes to one-fifth the original amount, and the storage of the river Lea water for fifteen days, by the East London Company, reduces the number on the average from 9240 to 1860 per cubic centimetre or to one-fifth; and lastly, the water of the New River Cut, containing on the average 4270 microbes per cubic centimetre contains, after storage for less than five days, only 1810, the reduction here being not so great, partly on account of the shorter storage, but chiefly because the New River Cut above the point at which the samples were taken, is itself a storage reservoir containing many days' supply after filtration. Indeed quietness in a subsidence reservoir is, very curiously, far more fatal to bacterial life than the most violent agitation in contact with atmospheric air; for the microbes which are sent into the river above the falls of Niagara, by the City of Buffalo, seem to take little or no harm from that tremendous leap and turmoil of waters, whilst they subsequently, very soon, almost entirely disappear in Lake Ontario.

It is not, therefore, too much to expect that storage for, say a couple of months, would reduce the number of

microbes in the Thames flood water down to nearly the minimum ever found in that river in dry weather, whilst, by avoiding the first rush of each flood, a good chemical quality could also be secured.

There is, therefore, I think, a fair prospect that the quantity of water derivable from the Thames at Hampton could be increased from its present amount (120 millions of gallons per diem) to 370 millions.

Again, in the river Lea, although here the necessary data for exact calculations are wanting, it may be assumed that the present supply of 54 millions of gallons could be increased by the storage of flood water to 100 millions per day. To these volumes must be added the amount of deep-well water which is attainable from those parts of the Thames basin which lie *below* Teddington Lock, and in the Lea basin *below* Lea Bridge, and which was estimated by the last Royal Commission at rather more than 67½ millions of gallons.

Thus we get the grand total of 537½ millions of gallons of excellent water obtainable within the Thames basin, the quality of which can be gradually improved, if it be considered necessary, by pumping from the water bearing strata above Teddington and Lea Bridge respectively, instead of taking the total supply from the open rivers above these points. Such a volume of water would scarcely be required for the supply of the whole water area of London at the end of fifty years from the present time, even supposing the population to go on increasing at the same rate as it did in the decade 1881-91, which is an assumption scarcely likely to be verified.

In conclusion, I have shown that the Thames basin can furnish an ample supply for fifty or more years to come, whilst the quality of the spring and deep-well waters and of the filtered river water would be unimpeachable. To secure these benefits for the future, storage must be gradually provided for 11,500 millions of gallons of flood water judiciously selected in the Thames Valley, and a proportionate volume in the basin of the Lea; whilst filtration must be carried to its utmost perfection by the use of finer sand than is at

present employed, and by the maintenance of a uniform rate during the twenty-four hours.

There is nothing heroic in laying pipes along the banks of the Thames, or even making reservoirs in the Thames basin. They do not appeal to the imagination like that colossal work, the bringing of water to Birmingham from the mountains of Wales, and there is little in such a scheme to recommend it to the minds of the enterprising engineers of to-day. Nevertheless, by means of storage, by utilising springs, by sinking deep wells, and by such comparatively simple means, we have, in my opinion, every reason to congratulate ourselves that for half a century at least we have *at our doors*, so to speak, an ample supply of water which for palatability, wholesomeness, and general excellence will not be surpassed by any supply in the world.

E. Frankland.

# SOME RECENT MEMOIRS UPON OLIGOCHÆTA.

THE literature relating to this group of worms is summed up in my *Monograph of the Oligochæta* lately issued by the Clarendon Press; but so energetic are the unfortunately somewhat few workers in this particular subject that new facts have gone on accumulating with some rapidity since the publication of that work. It is my intention in the present article to offer the reader a *résumé* of this latest work with, naturally, some references to what has gone before.

It is agreed by all those who are acquainted with the terrestrial Oligochæta that their peculiar mode of life, their susceptibility to sea water, and the comparatively few chances of dispersal enjoyed by them, render their distribution highly important in estimating the relations between land masses now and in the past. This has an especial bearing upon the theory of the former northward extension of the Antarctic Continent, a matter upon which much has been written lately. To deal adequately with this large question would of course demand more space than can be allowed me. I shall content myself with referring solely to the evidence which is forthcoming from the study of earthworms. Fortunately we are in possession of a considerable amount of information about the terrestrial Oligochæta of New Zealand and Patagonia; the former country indeed must be regarded as being better known perhaps than any quarter of the globe, excepting of course Europe. The extensive collections lately made by Dr. Michaelsen in South America have added largely to the number of species brought back by his predecessors. It results from an examination of the species found in the two countries that in both of them the prevailing types belong to the genera *Acanthodrilus* and *Microscolex*, particularly the former. Of the thirty-two indigenous species at present known from Patagonia and the more southern parts of the South Ameri-

can Continent, twenty are members of the genus *Acanthodrilus*, eleven are *Microscolex* and one is a *Perichæta*. Besides these are a few obviously imported *Lumbricus* and *Allolobophora* from Europe or North America. I say obviously imported because these worms are only found in cultivated ground and near the coast; as civilisation is left behind these species decrease and are replaced by the truly indigenous species. Among the twenty species of *Acanthodrilus* are included two or three which occur in the Falkland Islands and in South Georgia. Turning to New Zealand we find that out of twenty indigenous species nine are *Acanthodrilus*, six belong to the closely allied genera *Octochætus*, *Deinodrilus*, and *Plagiochæta*, three are *Microscolex*, while the two remaining are a *Perichæta* and a *Megascolides*, two genera which are eminently characteristic of the adjoining continent of Australia. Between New Zealand and South America is a long stretch of ocean, sparsely scattered over which are islands of volcanic origin. From three of these islands earthworms have been collected. In Kerguelen and Marion Island is a species of *Acanthodrilus* peculiar to those islands, and I have lately received, and am describing in the forthcoming June number of the *Proceedings of the Zoological Society*, a second species of that genus from Macquarie Island. The significance of these facts will be more apparent when we consider how far the genera that have been referred to in the foregoing are distributed outside of this antarctic area. *Microscolex* is found in many parts of central and the warmer western regions of North America; it has been met with also in Europe, Algeria and Teneriffe. *Acanthodrilus* occurs in Australia where it is represented by three species, all of which however inhabit the eastern half of the island continent, that part in fact which is nearest to New Zealand; *Acanthodrilus* has one species in Natal, one in New Caledonia and two in North America.

We have evidently therefore a fauna of earthworms peculiar to the antarctic region, into which more northern forms have been able to make but slight inroads and from which but few stragglers have wandered.

As to other distributional facts and theories, it is probable that I have underestimated in my Monograph the distinctness of the Palæarctic and the Nearctic regions of Mr. Sclater. I was disposed to unite them into one Holarctic as Professor Newton has called it. Further investigations have tended to emphasise the justice of separating these two regions. This evidence has been mainly collected by the industry of Dr. Gustav Eisen, of San Francisco; but others whose names and memoirs will be found quoted in the list of literature at the end of this article have added details of importance. The North American continent is inhabited by a fair number of peculiar genera, of which *Diplocardia*, originally described some years since by Garman, has four species (partly referred to the undoubtedly synonymous genus *Geodrilus*); there are also peculiar to this region *Phoenicodrilus*, nearly related to the central and South America *Ocnerodrilus*, and *Sparganophilus;* of this latter genus the original species was found by Benham in the Thames; but as there are half a dozen American species it seems likely that its occurrence in England is a case of importation. *Bimastos* is a genus perhaps justly separable from *Allolobophora*, from which it chiefly differs in the large size (for a Lumbricid) of the glandular sac in which the efferent male ducts terminate. Besides these peculiar genera are a few species of the Central and South American genera *Ocnerodrilus* and *Kerria*, and of the almost world-wide *Benhamia*. *Aleodrilus* is an Acanthodrilid that Eisen is disposed to separate from *Diplocardia;* two species of *Acanthodrilus* complete the list of non-European inhabitants of the North American Continent. But in addition to these are a number of *Allolobophora* and *Lumbricus*—the characteristic forms of the Palæarctic region—two or three of which are, however, so far as our present knowledge goes peculiar to North America. These facts perhaps justify the retention of the Nearctic region, and they are perhaps also significant in that the peculiar forms are western in range—a possible indication of their approaching extirpation by European species introduced by commerce.

The original indigenous forms, South American in character, may be regarded as having been gradually driven to the west by the encroachment of artificially introduced species. In other respects the geographical regions indicated by the distribution of earthworms agree fairly well with the generally received scheme of Mr. Sclater. The Ethiopian region is peculiarly distinct; the Neotropical is also nearly if not quite as plainly marked; but the Oriental fades into the Australian, and it is indeed not easy to separate them at all.

The only other matter affecting the distribution of earthworms with which I shall deal here is the question of oceanic islands. Our information upon the subject is not however by any means extensive; the largest collection made is due to the energy of Mr. Perkins, and has been described by me in a paper communicated to the Zoological Society. These worms were gathered in the Sandwich Islands, and belong to a number of species of which only two (and a doubtful third) have not been found elsewhere; these two belong to the genus *Perichæta*, a genus prevalent in tropical regions, especially of the old world. That the bulk of the species known from these and other oceanic islands are forms which have been in all probability introduced by accidental transference by man is rather what might be expected from the limited powers of independent travel possessed by these animals. There is at present no certain evidence that there are any truly indigenous earthworms in oceanic islands, with the exception of Kerguelen—a fact which as I have already hinted may be due to other causes.

To Linnæus only a single species of earthworm was known, his *Lumbricus terrestris*, now believed to have been a compound of more than one species. Grube in his *Familie der Anneliden*, published in 1851, reckoned up only forty-two earthworms, and of these one or two are now known not to be earthworms at all, and of the remainder many are unrecognisable or synonyms. Since that period the increase of new forms has gone on—of late with extreme rapidity; at the present moment we are acquainted with rather over 500 distinct and well char-

acterised species. And this estimate does not take into consideration subspecies or well marked varieties, and pays no attention to "species incertæ". Of aquatic Oligochæta 150 is about the number of known species; but this group is decidedly less known than the former. As with other groups of animals this great increase in the number of known species has added to our knowledge of anatomical fact, but rendered harder the formation of classificatory schemes. No indistinctness, however, has arisen to blur the perfectly sharp outlines of the group Oligochæta, no "intermediate" forms have been discovered whose relegation to the group is a matter of uncertainty or convenience. At the same time a few approximations in structure to the leeches on the one hand, and to the Polychæta on the other have been discovered; but these are in no case of first-rate importance. Perhaps the most remarkable is the description of the gills of the African genus *Alma*. This worm was originally described under that name by Grube in 1855. Thirty-four years later Levinsen, apparently in ignorance of Grube's paper, named a fragment of what was obviously the same worm *Digitibranchus*, and described in the same paper *Siphonogaster*, an Annelid characterised by a pair of long processes an inch or so in length, and of a spatula-like form arising from the eighteenth segment. These have been subsequently shown to be processes containing the outer section of the sperm duct which opens near to the extremity. Michaelsen showed that all these three worms are identical, and has thus been able to put beyond question the existence of a true earthworm[1] with branched retractile gills on the posterior segments of the body. It was not by any means clear from the earlier descriptions that the gilled worm was not a Polychæt. Among the lower aquatic Oligochætes there are at least three gilled forms, apart from *Dero* which has a circlet of ciliated processes, with vascular twigs lying round the anus. These forms are *Chætobranchus* of Bourne, and *Branchiura* and *Hesperodrilus branchiatus* of myself. In

[1] Structurally; in habit it is aquatic.

the two latter (which are allied to *Tubifex*) are contractile branchiæ, not branched however, on some of the posterior segments of the body. More numerous are indications of affinity with the leeches. I may, in the first place, refer to that group of parasitic Oligochæta, once placed among the leeches but now usually allowed to be true Oligochæta, for which Vejdovsky has proposed the name of Discodrilidæ on account of their posterior sucker. An American genus *Bdellodrilus* has lately been studied with care by Moore whose results entirely confirm the placing of the worms among the Oligochæta and their removal from the leeches. Their chief points of likeness to the Hirudinea are (1) absence of setæ; (2) existence of jaws; (3) presence of a sucker; (4) median unpaired character of reproductive pores.

The first and last of these characters are, however, found in a few undoubted Oligochæta, for instance, *Anachæta*, as its name denotes, has no setæ, and besides Mr. Moore describes large gland cells in *Bdellodrilus* which may represent setigerous cells of Oligochæta. As to the median generative pores they are very frequent among Oligochæta. The reproductive organs themselves are decidedly upon the Oligochætous pattern. The gonads are entirely free from their ducts, and there is a single spermatheca, a structure entirely wanting among the true leeches. The male ducts are two pairs, opening freely by ciliated mouths into the coelom and uniting into a common terminal atrium. Their arrangement recalls that of the Lumbriculidæ. The ovaries are proliferations of the coelomic walls and their contents escape to the exterior by a slit in the body walls lined by epithelium, a kind of rudimentary oviduct paralleled in the Enchytræidæ, and in the Eudrilid *Nemertodrilus*. There is nothing leech-like about the reproductive organs, excepting the terminal penis—a structure, however, which is also found in many Eudrilids and in some other Oligochæta. The conclusions of the author that the Discodrilidæ are Oligochæta slightly modified for a parasitic life is quite borne out by their structure. We may admit at the same time that this modification is in the direction of the leeches.

In addition to questions of relationship to other neighbouring groups, recent investigation has brought to light facts of interest in the anatomy of the Oligochæta which bear upon the mutual affinities of the families and genera into which the order is divided. In this direction the main discoveries of importance relate to the excretory system. In all the simple aquatic genera each segment of the body contains a single pair of nephridia ; as a rule these organs are wanting in the anterior segments, and Professor Bourne was unable to find any nephridia at all in *Uncinais littoralis.* The absence of nephridia in the anterior segments of the body, however, also characterises certain earthworms. It was originally described by Perrier in *Pontodrilus,* and all the species of this genus (6) are in the same condition. More recently Benham and Eisen have shown that the same state of affairs characterises the aquatic Geoscolecid *Sparganophilus.* A distinction therefore between the Limicolæ and Terricolæ of Claparéde quite breaks down. That these genera have no gizzard or calciferous glands (or at most the rudiments of a gizzard) is evidence of general degradation, which may have something to do with their aquatic or semiaquatic existence. It suggests too that the simplification in structure of the Limicolæ of Claparéde may be rather due to degeneration than to the retention of primitive characters.

Among the earthworms, however, the single pair of nephridia to each segment is far from being the rule. In a large number of genera the nephridia are multiple. Two pairs in each segment exist in *Brachydrilus ;* three pairs in *Trinephrus ;* and Eisen has lately shown that in certain North American Benhamias there may be three or four distinct and separate pairs each with its own internal funnel and external pore. The complexity of the excretory system culminates in *Perichæta* where a single segment may be furnished with probably at least one hundred external nephridiopores. It is, however, a question whether in this latter case there is really an intercommunication between the several nephridia of each segment, and between those of adjacent segments as has been alleged by Spencer

and myself. The matter requires renewed investigation. In any case Bourne, Vejdovsky and I have shown that the "plectonephric" condition, as Benham has termed these diffuse nephridial tubes, is preceded by a series of paired nephridia one pair to each segment. This has been proved in *Perichæta*, *Octochætus* and *Megascolides*. The nephridium elongates and becomes thrown into loops, each loop finally appears in *Megascolides* to break away and to form a distinct and separate nephridium. It is clear, therefore, that whether or not the connection is retained in *Octochætus* and *Perichæta* there is originally a connection, so that that matter is of less importance than the alleged intercommunication from segment to segment. This multiple arrangement of the nephridia is only found in the families Acanthodrilidæ, Perichætidæ and Cryptodrilidæ, and is the principal argument for uniting them into one superfamily, Megascolicides, as I have done in my Monograph. *Brachydrilus*, however, is a member of the family Geoscolicidæ, but it has only two pairs of nephridia to each segment; there is nothing like the complicated system of *Perichæta*. This family Geoscolicidæ has been through the recent researches of Rosa and Michaelsen brought still nearer to the Lumbricidæ. It was always difficult to separate them, mainly on account of the aquatic *Criodrilus*, now it is practically impossible unless we accept Michaelsen's intermediate family Criodrilidæ. The ornament setæ which used to be a distinctive mark of the Geoscolicidæ have been found by Michaelsen in *Allolobophora mœbii* and in *A. lonnbergi;* many Geoscolicidæ, *e.g.*, *Microchæta* are distinguished by the fact that instead of a single pair of spermathecæ in each of those segments which contain them there are a considerable number of minute pouches; this distinction, however, falls to the ground since more than one *Allolobophora* is now known to possess the same character—which has moreover been met with in *Perichæta*. It is in these two families that most instances are met with of total absence of spermathecæ; *Kynotus*, a Madagascar genus, is anteclitellian like the Lumbricidæ, and in short it seems impossible to lay down any

set of characters which should absolutely separate the two families. Several members of the two families are aquatic; thus among the Geoscolicidæ *Bilimba* (with which Michaelsen now suggests to unite Horst's *Annadrilus* and *Glyphidrilus*), *Criodrilus*, whose range the same author has lately extended to South America, *Alma* and *Sparganophilus*. Of Lumbricidæ *Allurus* is the only form which is often aquatic. Michaelsen has dwelt upon the fact that all of these, with the exception of *Sparganophilus*, have the body generally or at least the posterior region markedly quadrangular in outlines with the setæ implanted at the four corners. This is an apparent consequence or at least concomitant of aquatic life which is more curious than explicable. So much then for recent modifications of the systematic arrangement of the group. I shall deal finally with various anatomical and histological discoveries which have a general interest unconnected with systematic relations. The most important work under this heading is undoubtedly the recent investigations into the structure of the remarkable family Eudrilidæ, a well-defined family whose boundaries have not become in the least indistinct by the discovery of new forms. The family is remarkable on account of its distribution as well as on account of certain anatomical peculiarities. It is limited to tropical Africa—to the Ethiopian region of Sclater, with the sole exception of the type genus *Eudrilus*, whose ubiquitousness, however (America, West Indies, India and the East generally, New Zealand, etc.), makes one suspect direct transference by man. This family is chiefly interesting on the anatomical side by reason of the illustration which it gives of two phenomena, *viz.*, substitution of organs and change in function of organs.

In all Oligochæta the ovaries are paired (rarely unpaired) structures which arise from the peritoneal epithelium of the earthworms invariably the thirteenth segment. They are totally unconnected with the oviducts whose open mouths are placed exactly opposite to them. In the Eudrilidæ these gonads are enclosed in sacs which communicate with a system of sacs the complexity of

which varies in different genera, and of which it would be impossible to give any detailed account without the assistance of figures. There is a separate *receptaculum ovorum* like that of the common earthworm, with which is connected the oviduct. This system of sacs, through which the ova can travel in so far as there are no physical hindrances, also contain sperm, and play the part of spermathecæ or a spermatheca. They commonly open by a single ventral pore; sometimes the structures are paired as in the genus *Eudrilus* itself. Now these pouches generally contain sperm, and there is therefore the possibility of the ova being impregnated within them; Michaelsen has even suggested that some species are viviparous. In a few genera, for example in *Heliodrilus*, these pouches do not communicate with the exterior except through the oviducts. They appear to do so by a large ventral pore, but when careful sections are made it is found that this pore is the mouth of a closed sac, exactly like a spermatheca, which is enclosed within the large pouch. Thus the cœlomic nature of this system of sacs is established on anatomical grounds, and developmentally they have been shown, at least in one genus, to be derivatives of the intersegmental septa just as are the sperm sacs of other earthworms; their cavities are therefore separated portions of the general cœlom. But, as already mentioned, in most cases they do open on to the exterior directly by a conspicuous orifice, and contain sperm which probably finds its way into them by this orifice. The fact that in some cases these sacs contain structures which are precisely like the spermathecæ of other earthworms, and that in other cases where they open directly on to the exterior the character of the lining epithelium changes near to the orifice, becoming distinctly columnar, suggests that we have to do here with the substitution of sacs derived from the septa for the true spermathecæ which are gradually disappearing, only the extremity being left in the majority of cases. The second point with which I wish to deal concerns the calciferous glands. Most, but by no means all, earthworms possess one or more pairs of these organs, which are attached to and open into the œsophagus. What-

ever may be their functions they contain crystals of carbonate of lime, and have a rich vascular supply, the lining epithelium being much folded and therefore extensive. In some Eudrilidæ these structures are absent or rather are so altered that they are nearly unrecognisable as calciferous glands. At the same time they have become more numerous. The structure is altered in that instead of an extensive lumen produced by the folding of an excretory epithelium there is a very short sac connected with the œsophagus, which is, however, enveloped by an extensive coating of cells which I regard as cœlomic cells, and among which meander abundant blood-vessels. These cœlomic cells, where they abut upon blood-vessels, very often lose their oval or rounded form and become columnar and at the same time more darkly staining. They surround the blood-vessel as if it were the lumen of a secreting gland, the cells themselves having acquired the appearance of a secreting epithelium. These phenomena suggest that we have to do here with a change of function on the part of the calciferous glands; that their function of producing carbonate of lime, that their connection with alimentation has disappeared or is disappearing, and that a new function more intimately connected with the vascular system has supervened. There is a certain analogy here with the vertebrate liver which has certainly more functions than that of pouring bile into the intestine, though originally it may have been merely an annex of the alimentary canal.

In histology there is only one matter to which I shall direct the attention of the reader. It concerns the minute structure of muscular fibres in the Oligochæta. The careful researches of Cerfontaine have established the fact that the Oligochæta, like the leeches, have muscular fibres which consist of an outer sheath often radiately striated, the muscular substance, and a soft central core. Hesse, however, while admitting this, goes a step further and endeavours to prove a resemblance to the muscular fibres of the Nematoidea. He figures in the Enchytræidæ and in the Lumbricidæ a gap in the sheath of the fibre through which the soft less-modified protoplasm of the interior com-

municates with a pear-shaped nucleated body outside. If these observations prove ultimately to be correct it is clear that there is a close resemblance in this particular between the Oligochæta and the Nematoidea.

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# NOTES ON ATOMIC WEIGHTS.

IN a former article[1] a sketch of the state of our knowledge as to the relative atomic weights of hydrogen and oxygen was given. It was there shown that although the great mass of the evidence was in favour of the atomic weight of oxygen being about 15·88 times that of hydrogen yet there was a certain amount of experimental work by well-known and tried observers which seemed irreconcilable with this result, the chief paper (1) being that of Professor Julius Thomsen of Copenhagen, and based on the proportion by weight in which ammonia and hydrochloric acid combine to form neutral ammonium chloride. In a short paper by the late Lothar Meyer (2) it was proved conclusively how little value could be attached to a determination of this nature however accurate and careful the manipulative work might be.

Any hopes which might have survived in the minds of the most ardent follower of Prout, that the atomic weight of oxygen is exactly sixteen times that of hydrogen, must now be dispelled by the recent publications of E. W. Morley (3) and of Thomsen (4) himself. The work of Morley is so conclusive, and has been carried out with such untiring patience and skill, that to any one who reads the clear account which he gives of his methods and of the various checks employed, it must be quite evident that that type of worker of whom we regard Stas as the chief is not yet extinct, in spite of the prevailing view that one must publish as many papers as possible in the least possible time before one can be said to engage in "original research". Morley's scheme for the complete determination of the relative atomic weights of oxygen and hydrogen is a most ambitious one, and, although his results are quite conclusive now, it is much to be regretted that bad health and other circumstances over which he had no control (such as a workman pushing a brick through a wall on to a

[1] August, 1894.

delicate piece of glass apparatus) have up to the present time prevented him from carrying out his original programme in its entirety.

The paper consists of four distinct parts—

I. The determination of the weight of a litre of oxygen.

II. The determination of the weight of a litre of hydrogen.

III. The ratio by volume in which these two gases combine to form water.

IV. The synthesis of water from known weights of hydrogen and oxygen, the weight of the water formed being also accurately determined.

It would be impossible to give any idea of the precautions taken to obtain results free from all objections in a sketch so short as this must be, for such details the original memoir must be consulted; only a summary of the results obtained can here be given.

Three methods were adopted to determine the weight of a litre of oxygen. In the first method the barometer and thermometer were used, and the gases weighed in balloons holding in three of the experiments about 9 litres, and in the other six about 21½ litres.

In the second method a globe of pure and dry hydrogen was used as the standard for temperature and pressure, the globe containing the oxygen having its pressure determined at the same temperature as that of the hydrogen by means of a very sensitive differential manometer.

In the third method the globes were filled with oxygen when they were immersed in melting ice and the pressure accurately determined at the moment of closing. This method had the disadvantage of wetting the surface of the globes, and probably thereby changing their weight (although this was duly investigated).

The values obtained by these three methods for the weight of 1 litre of oxygen under normal conditions of temperature and pressure at sea level in lat. 45° were

| | |
|---|---|
| By use of thermometer and manometer - | $D = 1{\cdot}42879 \pm {\cdot}000034.$ |
| By compensation - - - - - | $D = 1{\cdot}42887 \pm {\cdot}000048.$ |
| By use of ice and barometer - - | $D = 1{\cdot}42917 \pm {\cdot}000048.$ |

From various considerations taking into account errors incidental to certain methods and liability to constant errors Morley gives the most probable value as 1·42900 ± 0·000034.

In the same way experiments were made with hydrogen and in five series but practically by three methods.

First method was practically the same as the first series of oxygen experiments.

Second method was like the third oxygen series.

Third method utilised the power of absorbing hydrogen possessed by palladium. The hydrogen was weighed in the palladium and expelled into globes, and its volume and pressure determined at the temperature of melting ice. Series III., IV. and V. were made by this method, but the apparatus employed varied somewhat in the various series.

The values which result from these experiments are

Series I. $D_h$ = ·089938 gram.
Series II. $D_h$ = ·089970 gram.
Series III. $D_h$ = ·089886 ± ·0000049 gram.
Series IV. $D_h$ = ·089880 ± ·0000088 gram.
Series V. $D_h$ = ·089866 ± ·0000034 gram.

The higher results of Series I. and II. are possibly due to some constant error, probably traces of mercury vapour. The most probable value is

$D_h$ = ·089873 ± 0·0000027 gram.

Part III. of the paper begins with a sketch of the methods it was proposed to employ to determine the volumetric composition of water. Of the three methods proposed Morley unfortunately has only been able to carry out the one which is the least satisfactory, *viz.*, the determination of the density of electrolytic gas and of the excess of hydrogen over and above what the oxygen can unite with. Leduc made a similar density determination, but apparently assumed that the hydrogen and oxygen were in the exact proportions in which they would recombine to form water. Morley found that he always had an excess of hydrogen when he kept his voltameter in ice and water, but that when the temperature was allowed to rise to about 20° C. then oxygen was in slight excess, so that no doubt at a

certain temperature the gases do come off in atomic proportions. In each experiment the weight of the gases given off was about 23 grams.

The weight of a litre of the gas thus given off from solution of soda made from clean sodium was—

$$0{\cdot}535510 \pm 0{\cdot}000010,$$

and corresponds to a mixture of one volume of oxygen with 2·00357 volumes of hydrogen, but the excess of hydrogen was found to be ·00088 giving therefore the ratio in which the gases combine as 1 : 2·00269.

Part IV. gives an account of experiments in which hydrogen was weighed in palladium foil, oxygen was weighed in a globe, these were then made to combine, and the water produced was weighed also.

From these experiments we get the following values for the atomic weight of oxygen :—

| | |
|---|---|
| (1) From the ratio of hydrogen and oxygen, - - | 15·8792 |
| (2) From the ratio of hydrogen and water, - - - | 15·8785 |
| or as a mean, - - - - - - | 15·879 |

From Parts I., II., III. of the memoir we get

$$\frac{1{\cdot}42900}{{\cdot}089873} \times \frac{2}{2{\cdot}00269} = 15{\cdot}879$$

How excellent Morley's work is can perhaps best be seen by comparing his results with the means of those of previous experimenters,

| | Rayleigh's summary. | Morley. |
|---|---|---|
| Density of oxygen at Paris, - - | 1·42961 | 1·42945 |
| Density of hydrogen at Paris, - - | ·08991 | ·089901 |

| | |
|---|---|
| Ratio of densities mean of all previous determinations, - - | 15·9005 |
| Ratio of densities, Morley's, - - - - - - - | 15·9002 |

| | |
|---|---|
| Ratio of combining volumes, Morley, - | 2·00269 |
| ,, ,, Scott, - | 2·00285 |
| ,, ,, Leduc, - | 2·0037 (corrected = 2·0024) |

Although the results obtained by Thomsen agree wonderfully well with those of Morley it is not because his apparatus and his methods of working are so carefully elaborated. On the contrary what strikes one most forcibly is the extreme simplicity of the apparatus and mode of

working it as well as the neglect of certain precautions which could well have been taken, and ought to have been taken in an attempt to settle such an important constant as the present; such precautions as to weighing with counterpoises of equal volume, for example, seem to have been neglected.

The method was to determine, firstly, the weight of hydrogen given off from unit weight of aluminium when dissolved in strong potash solution; secondly, by supplying oxygen to a small combustion chamber so as to burn the hydrogen evolved from a known weight of aluminium, and collect all the water formed in the apparatus, one gets thus the gain of the equivalent amount of oxygen to the hydrogen and to the aluminium. The only corrections not of the simplest order were due to the oxygen and hydrogen remaining in the apparatus or which had to be evolved after the combustion had ceased. It was not found possible to burn all the hydrogen evolved completely as the current became so very slow when a very little aluminium remained undissolved. The aluminium did not require to be perfectly pure as long as it gave off no other gas than hydrogen. It was found that 162·3705 grams of aluminium gave off 18·1778 grams of hydrogen giving the ratio

$$\frac{\text{Hydrogen}}{\text{Aluminium}} = 0{\cdot}111902 \pm {\cdot}000015$$

as the mean of twenty-one experiments.

The weight of oxygen required to combine with 86·9358 grams of aluminium (or rather with the hydrogen evolved by its solution in potash) was found to be 77·1876 grams from which we get the ratio

$$\frac{\text{Oxygen}}{\text{Aluminium}} = {\cdot}88787 \pm 000018$$

from which two results we get

$$\frac{O}{H_2} = \frac{{\cdot}88787}{{\cdot}11190} = 7{\cdot}9345$$

$$\text{or } \frac{O}{H} = 15{\cdot}8690 \pm {\cdot}0022$$

We seem to have every reason now to regard it as completely proved that the atomic weight of oxygen is 15·87 to

15·88 times that of hydrogen, the higher value being in all probability the more correct.

Having now satisfactory determinations of our fundamental ratio we still require other ratios to be able to determine conveniently the atomic weights of many elements. If an element forms many compounds with oxygen it is never safe to conclude without the most rigorous proof that we have a pure oxide absolutely free from the other oxides of the same element. Hence determinations of atomic weights made by the reduction of oxides to the element or of one oxide to a lower one or of the oxidation of an element to an oxide or of one oxide to a higher oxide must always be accepted with caution. The use of the haloid compounds (especially those of bromine), of many elements, is of the greatest value, and for this we require an exact knowledge of the ratio bromine : oxygen. For this we depend chiefly on the classical work of Stas. The publication of the complete works (5) of J. S. Stas under the able editorship of Professor W. Spring, of Liége, enables every one now to obtain in an elegant and convenient form these models and masterpieces of accurate research which were formerly so difficult to procure. How great the contrast between the work of Stas and too much of that turned out at the present day a glance at almost any page of his works will show. Every step was proved most conclusively, however simple and even axiomatic it may seem to us now, before he proceeded to more elaborate propositions and deductions. For instance, in his *Nouvelles Recherches* he begins by proving that ammonium chloride prepared from absolutely different sources and purified in different ways always contains exactly the same proportion of chlorine, and that the same weight of each sample precipitated exactly the same amount of silver from its solution in nitric acid. He obtained his ammonia from ordinary *sal ammoniac* after destroying any organic bases by a treatment with *aqua regia*, and from commercial ammonium sulphate by a similar purification, by heating it to a high temperature with strong sulphuric acid, and then oxidation with nitric acid, and from potassium nitrite by reduction in an

alkaline solution with purified metallic zinc. The ammonium chloride was sublimed now in a current of ammonia gas, now *in vacuo*, but the results obtained showed that for the complete precipitation of 100,000 parts of silver, 49,592 to 49,602 parts of ammonium chloride were required. In other words, the *extreme* difference in a large number of determinations carried out with very considerable modifications only amounted to one part in five thousand.

Having thus proved that a compound always contains the same proportion of its constituent elements it was essential for his purpose as well as for the complete establishment of the atomic theory to prove that the equivalent weight of an element was not affected in the slightest degree by the various elements with which it might combine. To take an example, silver combines with iodine to form the iodide, and with iodine and oxygen to form the iodate, and these compounds are represented by the formulæ $AgI$ and $AgIO_3$ respectively. It was just possible, one might even say probable, that the ratio of silver to iodine in the one compound might not be the same as that in the other, but that it would be modified by the large quantity of oxygen present in that other substance. If, however, the elements consist of small particles alike in all respects, such a variation would be impossible, and the relative masses of silver and iodine in the iodide and in the iodate must be absolutely the same. To prove this may seem very easy, but Stas found it by no means so, for whenever he prepared his silver iodate by precipitation from the nitrate, after the reduction with sulphurous acid there was always a small excess of silver over and above the iodine present. This he finally traced to a minute quantity of the nitrate being carried down mechanically by the iodate, but so firmly held that no amount of washing would remove it. By using other soluble salts of silver such as the sulphate and the dithionate, however, he was able to prepare silver iodate so pure that on reduction to silver iodide not the slightest trace of either silver or iodine remained in excess. In the case of that prepared from the nitrate the excess of silver only amounted to one part in

3,000,000. These simple experiments give us some idea as to how hard it is to obtain even very simple compounds in a state of absolute purity. Having thus laid the foundations for his further work, and shown that the combining proportions of elements are mathematically exact, Stas considered no labour too great if thereby he could obtain more accurate values for these proportions. Any work done since his determinations has only tended to uphold his values and to increase our admiration for his work.

The great value of very accurate experimental work has been most strikingly exemplified by Lord Rayleigh's determinations of the density of nitrogen (6). He found that the nitrogen which he could obtain from air alone by removing the oxygen was *very little* denser, but was *always* denser than that prepared from the air with the aid of ammonia by Harcourt's method, and that the nitrogen prepared from ammonia or from any compound had always the same density, and that this was still lighter than that partly from air and partly from ammonia. From this he concluded that besides nitrogen the atmosphere must contain another constituent still denser, which like nitrogen resisted the action of iron and copper as well as their oxides, even when very strongly heated. By combining the nitrogen with oxygen after the method of Cavendish, or by causing the nitrogen to unite with metallic magnesium, a new gas to which the name of argon has been given was finally separated by Rayleigh and Ramsay after much laborious work. The detection in the atmosphere of a constituent hitherto unsuspected as well as its isolation are apparently only the first fruits of a number of more or less startling discoveries flowing directly from Lord Rayleigh's very accurate work. The molecular weights of argon (7) and helium (8) are respectively 40 and 4, and if their molecules are monatomic this would give us the same numbers for their atomic weights, but if the molecules are diatomic, as is probable, these numbers would be halved for the atomic weights. It is far from certain that either what we call argon or what we call helium is not a mixture of several similar substances.

Several atomic weights have been redetermined with great care, and of these determinations perhaps those of T. W. Richards of barium and of strontium are the most accurate and most interesting. By an exhaustive research on barium bromide he deduces the value Ba = 137·434 (O = 16) (9). From a similar study of barium chloride the value Ba = 137·440 is deduced (10).

This value is notably higher than that usually accepted and is no doubt due to the careful elimination of small quantities of strontium and calcium which have contaminated the preparations of earlier experimenters. From a study of strontium bromide Richards found Sr = 87·659 (O = 16) (11).

Still more recently the atomic weight of zinc has been determined by Richards and Rogers again by means of the bromide and precipitation with silver, and as a mean they find the value (Zn = 65·404) (O = 16) (12).

In all the above determinations Richards estimated the percentage of silver in his haloid silver salt and showed it to be identical with that found by Stas, thus placing his work on the same footing and guaranteeing in this way its very high accuracy.

In 1888 two other American experimenters, Burton and Morse (13), published the results of their work on the same atomic weight which they arrived at by means of the conversion of the metal into the oxide by treatment with nitric acid and ignition of the nitrate. Although their work agrees throughout very well the value found is lower than that of Richards, due no doubt to the retention by the oxide of oxides of nitrogen as Marignac pointed out. In defending their work against this objection they expose their want of knowledge of the commonest reactions in such a way as to make one distrust all their work. The perusal of their paper provides much food for reflection of a serious nature although it does give a certain amount of instruction as well as amusement. They carry out their weighings to ·00001 of a gram and pretend to detect differences of this minute amount in porcelain crucibles which have been heated up to the melting point of steel. In their account

of the purification of metallic zinc by distillation *in vacuo* it is rather odd to find it stated that indiarubber tubing with glycerine joints could not be used because *the vapours of zinc and of glycerine interact.* What pressure of the vapour of each is likely to exist at the highest temperature to which the joints would ever be subjected? The presence of gold in the nitric acid distilled from a platinum still, and coming from the gold solder used in it sounds also rather peculiar. One knows that very finely divided gold will dissolve in fuming nitric acid if kept cold, but one could hardly have thought of finding it as an impurity in nitric acid prepared by distillation. But the gem of all the statements comes at the end of the paper when these two rising experimentalists proceed to criticise Marignac's work (14), and finally to teach him and us how we ought to test for oxides of nitrogen by means of starch and potassium iodide. After proving to their own satisfaction by a process which cannot reveal the presence of any of these oxides that they are therefore obviously absent, they conclude that Marignac was ignorant of the necessary precautions which must be taken to exclude oxygen, especially that of keeping the solution practically boiling so that the steam may keep out the air. It is usually accepted as a well-established fact that the delicacy of this reaction decreases rapidly with rise in temperature, and that the colour goes completely before the boiling point is reached, even in the presence of relatively large quantities of free iodine.

Amongst other noteworthy determinations of atomic weights made recently are those of Winkler, who finds the values Ni = 58·91 and Co = 59·67 by means of the reaction between the chlorides and silver (15); and still more recently Ni = 58·71 and Co = 59·37 (16) by determining the amount of iodine required to unite with the pure metal. Winkler uses the value Ag = 107·66, if we use O = 16 or Ag = 107·93 these last values become

$$\begin{aligned} Ni &= 58{\cdot}863 \\ Co &= 59{\cdot}517 \end{aligned}$$

The determinations of the atomic weight of boron by

Ramsay and Acton (17), as well as by Rimbach (18), are very interesting as examples of various methods of attacking this problem, and which give fair results, but they can hardly be said to have given results possessing greater accuracy than those of Abrahall (19).

Of all the elements of which the atomic weights are still in doubt, and of which the determinations are very unsatisfactory, by far the most interesting is undoubtedly tellurium. According to the periodic classification of the elements it ought, as is well known, to have an atomic weight less than that of iodine, but all the most satisfactory determinations are irreconcilable with this, and make the atomic weight notably higher than that of iodine. The experiments made in recent years both by Brauner (20) and by Wills (21) agree in this, no matter what method is adopted as long as it is one which gives concordant results. The latest determinations, those of Staudenmeier (22) which start from telluric acid, give, according to him, the values 127·6, 127·1, and 127·3 for three series of experiments in which different ratios were determined. He takes as his standard O = 16 and H = 1·0032. Staudenmeier upholds that tellurium is an element in opposition to Brauner who at one time maintained that it was a mixture of true tellurium with a higher homologue, but now concludes that this is very improbable, and since the discovery of argon suggests that the assumed impurity may be a homologue of argon. Speculations of this nature are strongly to be discouraged and condemned, especially when their basis is nothing more than the assumed abnormality in the periodic arrangement of the elements coupled with a very decided want of agreement in the results of an experimenter's own work obtained by different methods. They may afford an easier way out of a difficulty than by working steadily at the causes of such discrepancies, but afford at best but a feeble and undignified cover for one's retreat.

*P.S.*—About the middle of last month, and after the above article was written, Thomsen (23) published the results of some new determinations of the densities of oxygen and hydrogen. The oxygen was prepared by

heating a mixture of potassium chlorate and ferric oxide, and the hydrogen from a solution of caustic potash by the action of metallic aluminium. The values found were :—

Weight of one litre of oxygen at 0° C. and 760 mm. pressure, at sea-level in Latitude 45° - - - = 1·42906 grams.
And of hydrogen similarly - - - - = ·089947 gram.

From these he deduces the ratio of the volumes in which they must combine to form water to be 1 : 2·00237.

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# THE STELAR THEORY; A HISTORY AND A CRITICISM.

## PART II.

### THE METAMORPHOSES OF THE STELE.

IT is clear that the theory of polystely forms an integral part of the general stelar doctrine, and we can hardly refuse to accept its main idea. But though each stele in the polystelic stems of, for instance, *Auricula Ursi* and many Polypodiaceæ is clearly the equivalent of the whole cylinder in the hypocotyl of the same plants, cases exist in which we seem forced to consider as steles, vascular strands which have none of the characters of the cylinder left about them.

Deriving our idea of the typical stele from the monostelic organ, we come to consider it as essentially cylindrical and radially symmetrical. It is true that diarch roots are bilateral in structure, and the primary root and hypocotyl of very many ferns being diarch the steles of a great number of their stems are likewise diarch and hence bilateral. And this bilaterality often extends to the *shape* of the stele which becomes oval or band-shaped instead of circular in transverse section, the two protoxylems being situated at the extremities of the figure. Another step is for the stele to become more or less semilunar in transverse section, so that it is no longer symmetrical about the plane passing through the protoxylems, but only about the bisecting plane perpendicular to this. And further the protoxylems may lose their symmetrical arrangement, or one only may be present, and this may be excentrically placed (*Angiopteris*). We clearly could not tell that such strands were steles if we had no knowledge of their connexions and disposition. At least as far as tissue arrangement goes they may often be said to have lost those characters which entitle them to the name. A similar difficulty meets us in the case of the vascular strands in many fern leaves. Undoubted steles found in

the petiole, after repeated branchings gradually lose the phloem from their upper sides, and thus come to possess the collateral structure of the bundle of a Phanerogamic leaf. On the other hand the curved bundle in the petiole of *Osmunda* is certainly a meristele, if we may judge from its connexion with the bulky central cylinder of the monostelic stem, yet it is surrounded by a complete mantle of phloem, and indeed conforms in structure to many true steles (*cf.* 18). We may probably draw the same conclusion as to the "petiolar steles" of Gleicheniaceæ (19).

Similar facts appear to obtain in the polystelic genera of Phanerogams, upon which we may expect much new light from as yet unpublished researches. One instance is, however, too instructive to be omitted. A number of distinct steles arranged in a circle enter the peduncle of *Auricula Delavayi* (8, p. 304), fuse laterally, and become indistinguishable from a monostele, the central extra-stelar tissue passing over into pith.[1] Van Tieghem warns us (10, p. 768) not to confound such a structure formed in an essentially polystelic stem with an essentially monostelic stem. But if this sort of thing may occur, what guarantee have we that an "essentially monostelic" stem is really essentially monostelic, or, for the matter of that, that an "essentially polystelic" stem is really essentially polystelic? If a stele can become a collateral bundle in the course of a shoot system, the same transformation may very well occur, or a collateral bundle may become a stele, in the course of descent; at least we are quite debarred from dogmatically drawing or denying homologies between the one and the other. Of course we can speculate, and in some cases claim a fair degree of probability for our speculations, especially when we have a minute knowledge of all the facts in the anatomy of a given group, but since it is impossible to draw a sharp line between a stele and a vascular strand that is not a stele we are clearly not on very firm ground. There is certainly nothing to surprise us in this;

[1] A similar state of things appears to obtain in some of the Palm roots investigated by Mr. Cormack.

the instructive fact is that "there's such divinity doth hedge" a stele—indeed any morphological conception, as in almost every fresh case to prevent for a time our realisation of the truism that "Nature knows no sharp boundaries". In the stelar doctrine, we have, no doubt, a classification that enables us to perceive a little more closely the directions along which the various types of vascular system in the higher plants have been evolved, and that after all is the most we can expect.

## DEVELOPMENTAL EVIDENCE BEARING ON THE STATUS OF THE STELE.

We have now to consider the developmental basis of the stelar theory. Let us take the Phanerogams first. It is well, as Dr. Scott (20) has already pointed out in this journal, to draw a distinction between development from the embryo, and development of the various axes from their permanently embryonic growing points. It is clear, on reflection, that the former alone is comparable to ontogenetic development in animals, though it would be a mistake to suppose that the latter is not of importance to morphology. In the comparatively few types of monostelic plants with the anatomy of whose embryos we have a sufficient acquaintance, it appears that both in the plumule and radicle there is really a clear separation at the apex between central cylinder and cortex (plerome and periblem). But it is certainly open to doubt whether this distinction, as Hanstein thought, is really maintained at the growing points of the various axes throughout the life of the plant. Into the history of the differences of opinion on this point we need not enter. The inherent difficulties of arriving at valid conclusions from observations have been nearly as powerful as the subjective causes which have evidently influenced the views of the observers in creating the extraordinary discrepancies which exist between the various accounts.

The method employed by Ludwig Koch (21 and 22), who recognised that the state of things at the growing point was likely to differ at different epochs of growth, and

that hence conclusions drawn from observation of a few sections could not be final, marks a great advance on previous work. Koch claims to have proved (22), in *Syringa* and *Berberis*, that the single layer of cells immediately beneath the dermatogen, *i.e.*, the periblem of earlier observers, divides periclinally, during a period of leaf formation, across the actual apex of the shoot, thus giving rise to three or four superposed layers of cells. It is clear that, if this is the case, all but the uppermost of these layers must become part of the plerome when the apex passes back to the state of possessing a single layered periblem. But though our author has convinced himself that this actually happens, his figures are not decisive. Most of the periclinal divisions which he shows in the periblem of the Lilac (Taf. xvi.) are clearly in connexion with the formation of the leaf rudiments. In no case are such divisions shown across the actual apex. In fig. vi. periclinal walls are drawn in two periblem cells removed by one cell from the cell-group obviously concerned in the formation of a leaf rudiment, but these walls are also removed by one or two cells from the centre of the flat growing point, and considering how much this free surface is encroached upon by the developing leaves (*cf.* fig. vii.) it is not at all clear that the periclinal wall in question is not precociously formed in a cell which will later be involved in the base of the leaf. Yet this single periclinal wall is really the sole evidence obtainable from his figures of the truth of Koch's view. Nevertheless the thorough method of investigation inaugurated by Koch must sooner or later settle the point. For the present we must admit that though Hanstein's case is made out for a certain small number of plants, the great majority of cases which have been investigated must remain doubtful. Van Tieghem (10, p. 776) does not definitely commit himself, though he implies the suggestion that Hanstein's three initial layers are universal in Phanerogams, though often not distinguishable owing to "enchevêtrement" of the layers. But his pupil Douliot (23) concluded that there was a single apical cell in all Gymnosperms, and a plero-periblem in most

monocotyledons and some dicotyledons, while Koch takes the view that there is a generalised meristem without separate layers in Gymnosperms (21) and that only the dermatogen is separate in most Angiosperms (22). So that the "triple layer" theory of Hanstein and Van Tieghem is accepted by neither of these two most recent investigators as of general application, widely divergent as are their views *inter se*. Considering that the theory of the direction of ontogeny by the separation of different kinds of somatic idioplasm is now generally discredited, it is difficult to see what we gain by an adherence to the unproved hypothesis of the strict separation of the initial layers, even if it is still a possible hypothesis.

In the root apex on the contrary the plerome is in the great majority of cases sharply separated from the periblem, but even this rule is not universal. The sharp separation seems to be correlated both in root and stem with the formation of a slender compact cylinder.

In Vascular Cryptogams, which nearly all possess either a single apical cell or a single layer of initial cells giving rise to the whole of the tissue of the axis, there is of course no question of a separation, at the apex itself, of initial layers.

The separation of the young cylinder behind the actual growing point is quite a distinct question from its separation at the apex. It is during the development of the cylinder that we get, usually at least, a distinct limit between it and the cortex which is often lost in the adult stem, and this is a point of great importance.

Long before the stelar theory was originated, most of the great anatomists, who laid the foundations of our knowledge of the histology of vascular plants, were practically agreed on the generality of this early separation. This is clearly shown in the terminology employed in designating the various regions.

Thus Sanio (24), tracing from the apex the development of the various tissues, showed that in many cases the young pith first became separated from an outer zone, and that in the latter the "thickening ring" (really corresponding to

Flot's "vascular meristem," *i.e.*, the ring of tissue producing the bundle system *plus* the "external conjunctive": shortly became differentiated from the peripheral zone or young cortex. In other cases (*Euonymus* and *Berberis*), the "thickening ring" appeared or began to appear *before* the young pith became separated from the "outer zone". Hanstein (25), as a consequence of his separation of the primary meristem into Dermatogen, Periblem and Plerome, makes the outer limit of the young cylinder, *i.e.*, that between periblem and plerome, of primary rank. Russow's scheme (26), on the other hand, drawn from instances like those of Sanio's first group,[1] in which the young pith is the first tissue to become apparent, divides the young tissue produced by the general Protomeristem at the apex itself into *Endistem* (Sanio's young pith) and *Existem* (Sanio's "Aussenschicht"), the latter being separated into *Mesistem* (Sanio's "thickening ring") and *Peristem* or young cortex. Thus the limit between "Mesistem" and "Peristem" is reduced to secondary rank. But De Bary (14, pp. 395-6) again sums up clearly in favour of the individuality of the plerome.[2] As a matter of fact the young pith often does

[1] Russow placed Hanstein's best instances, for example, stem of *Hippuris*, and Roots, where there is a well-defined plerome at the apex itself, under the separate heading of "Axes with Combined Bundles".

[2] The development of the pericycle is of great importance in this connexion. Sanio (24) showed in several cases that what we now call the pericycle was developed from the outer edge of the "thickening ring". Schmitz (27) confirmed this view in *Berberis* and *Menispermum*. Van Tieghem, however (5), based his conception of the pericycle entirely on the ground of adult comparative anatomy. This is explicitly stated (p. 152) in a remark he made at the close of a "Note sur le pericycle," read by D'Arbaumont (28) to the Botanical Society of France. D'Arbaumont had endeavoured to show that the sclerised portions of the pericycle, capping the phloems of the stem bundles in dicotyledons, were developed in common with the bundles themselves from the desmogen strands, and were thus often separate from the interfascicular pericycle. His account of the development of the continuous zone of fibres in Cucurbitaceæ and in *Berberis* is different, and indicates differences in the origin of the pericycle in various plants. It is unfortunate that no figures are given. Morot replied (29) that even if the pericycle, or parts of it, were developed differently in different plants, that made no difference to the validity or applica-

become recognisable in comparatively bulky apices (owing to the early ceasing of longitudinal divisions, and the stretching of its cells), before the outer limit of the young cylinder is defined. On the other hand, in the slender stems of many water plants, Hanstein's scheme applies with diagrammatic precision, the outer limit of the cylinder being clearly marked at the apex, before there is any sign of a differentiation between pith and bundle ring. But these differences of precocity in the development of the various regions of the cylinder, depending, as they do, upon the subsequent duration and size relations of the regions are clearly of little importance to morphology. The important fact which remains is the clear separation, slightly sooner, or slightly later, of the young cylinder from the cortex, in at any rate the vast majority of cases.

The separation thus made in development is, as a rule, more or less clearly maintained in the adult stem, though sometimes it is lost altogether. There is the possibility of a complete loss of a visible boundary between cylinder and cortex by the occurrence of irregular cell divisions in the young pericycle and inner cortex, together with a "shifting" (Verschiebung) of the original walls separating the two; unfortunately we do not know if this takes place in some cases or not. But apart from such an occurrence the distinction between cylinder and cortex, once made, is always made, and the layer of cells which once abutted on the young cylinder is still the phloeoterma, not merely "theoretically," but in substance and in fact, however impossible it may become to distinguish it from the surrounding tissue.

It is these facts which form the real developmental basis of the stelar theory.

The phenomena (supposing them to be established) of real importance in the opposite sense, would be the occurrence of stems in which the external limit of the cylinder is never clear, of stems, in a word, which never possess a

tion of the term. The further pursuit of the theoretical implications of this statement would lead us into very deep waters, but it is clear that an extended comparative investigation of the origin of the pericycle is needed.

cylinder as such. While we could not admit that the stelar doctrine applied to such stems, we should probably be forced to the conclusion if their vascular system conformed in all other respects to the monostelic type, that the plants in question were derived from truly monostelic ancestors, whose descendants had lost the limit between cortex and cylinder.

The Nymphæaceæ, many of whose stems contain a large number of "scattered" bundles, seem to furnish us with examples of such plants. Caspary (27) states that the bundles are here developed in centripetal order: this would seem to indicate an analogy with those plants (Piperaceæ, Begoniaceæ, etc.), which possess a proper bundle ring and also younger bundles in the pith, rather than with the monocotyledonous type. In at least one member of the family, *Victoria regia*, which possesses a particularly large number of these "scattered" bundles, it appears that no well-defined cylinder is visible anywhere in the stem.[1] So here if anywhere we seem to have a real case of "astely". We cannot, however, say the same with certainty of any dicotyledonous stem with a single ring of bundles. Nägeli's observations (28) indeed led him to the conclusion that the "cambial" strands were, as a rule, developed in the midst of a homogeneous ground tissue, but his conclusions, as we have seen, have been negatived by most subsequent observers.

Turning to the vascular cryptogams we find that whether monostelic or polystelic, the stele or steles can be traced nearly up to the stem apex. The first formed periclinal walls do not indeed necessarily mark the limit of stelar tissue. They may cut off the pith, as in *Equisetum* or mark the middle of the cortex, as in many roots, or the outer limit of the ring of steles, as in many fern stems, or of the single cylinder, as in the stolon of *Nephrodium* (10, pp. 692 and 773-4). Clearly no special importance can be attached to these walls, and we certainly cannot use the fact that they mark off the pith in *Equisetum*,

[1] I owe this information to the kindness of a friend in telling me the results of some unpublished observations.

as Van Tieghem does, to support the view that the genus is really astelic. This argument depends on the assumption that these walls always separate stelar from extra-stelar tissue, which is not a fact, according to Van Tieghem himself (10, p. 774), and further, a similar line of reasoning would tend to show that the stems of a great many dicotyledons, namely, those in which the pith is the first tissue to be marked off, are also astelic.

## SUMMARY OF RESULTS.

We have attempted in the foregoing pages to exhibit, as clearly as possible, the bearing of well ascertained facts of anatomy and development upon the stelar theory as developed by Van Tieghem and his pupils. We may appropriately conclude with an attempt to summarise the results to which we are thus led.

We recognise in the central cylinder of the axes of the great majority of the higher plants an anatomical region of the first rank to be co-ordinated with the other great anatomical regions, the cortex and the epidermis. The central cylinder consists of vascular tissue (xylem and phloem) and conjunctive tissue (typically parenchyma). In the bulky typical[1] cylinder the vascular tissue is separable into distinct strands corresponding with its centres (or rather lines) of development, and giving to the cylinder a radial symmetry; the conjunctive of such a cylinder is separable into distinct regions. Typically, also, the innermost layer of cortex, which abuts on the cylinder is distinguished by special characters.

Reduced central cylinders are found in various stem structures, especially the thin stems of water plants. The reduction acts first on the conjunctive, which may (though rarely) quite disappear. This leads to the coalescence of the strands of vascular tissue into a more or less solid cylinder. Such a reduced cylinder is always sharply marked off from the cortex.

On the other hand we have stems in which it is impossible to separate the conjunctive from the adjacent

[1] In Sach's sense of " most highly developed ".

cortical tissue. When this is the case in the adult, it is still often possible to make the separation in the young stem.

Naming the central cylinder a *stele*, we call all stems with a single cylinder *monostelic*.

Stems in which we cannot make the separation in any part, and which are therefore not strictly monostelic, yet conform more or less to the monostelic structure in other respects, and are no doubt usually derived in descent from the monostelic type.

Most Ferns and Selaginellas, and two genera of Phanerogams, while showing a monostelic structure in their hypocotyls, possess in their later formed stems more than one cylinder, each comparable in structure to the single stele of the hypocotyl. Such stems are known as *polystelic*. The steles of a polystelic stem may, however, take on the most various forms, and lose all the characters of the original cylinder; several may even coalesce to form a structure indistinguishable from a single stele. As this, or indeed the converse case of a non-stelar vascular strand assuming the characters of a stele, may have happened in descent without leaving any traces of the transformation, we are not justified in asserting the homology of all steles or denying homology between steles and non-stelar vascular strands. Nevertheless the stele is undoubtedly a real and relatively stable type in the arrangement of vascular tissue, and hence the name represents a real morphological conception.

The vascular tissue of a leaf is arranged in one or more strands, each of which, bilaterally rather than radially symmetrical, is called a *schistostele* or *meristele*, representing, as it does, a part only of the stem cylinder. The meristele of a petiole may, however, simulate a stele. In most polystelic stems one or more of the stem steles directly enters the petiole, and the branches maintain more or less of the stelar character till near their endings in the lamina, where they become indistinguishable from collateral bundles.

We are probably justified in supposing the monostelic type to be primitive in vascular plants, and we may assume the original stele to have been relatively simple. To the increase in bulk of the stem and correlated increasing de-

mands for the supply of vascular tissue to leaves, the plant either responded by increasing the bulk of the stele and multiplying the number of its vascular strands, or by substituting a number of simple steles for the original single one. This last occurrence happened once at least in the Pteridophyta (probably more often), and more than once among the Phanerogams.

The primordial stele is represented at the present day by the single sharply defined stele of the embryo, which is maintained in the root and hypocotyl, and which passes over in the stem to one of the modern types of structure, necessary to the various demands of the leafy shoot. The arrangements at the apex of the latter are naturally adapted to form the particular type of structure in question, and can in no case be considered as representing an ancestral form.

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## THE PRESENT POSITION OF THE CELL-THEORY.

### PART II.

SINCE I have shown that protoplasm in the simplest form in which it is known to us may not be regarded as having an organisation in the sense in which that term has any meaning, and since it is a waste of time to discuss the use of the term when it has no meaning, we may more profitably turn to the question whether protoplasm has a structure, and if so, what kind of structure? Is it essentially the same in all the kinds of protoplasm which have been studied, and is it of the same kind as the structure of tissues and organs of metazoa or is it of a different kind? For it must be insisted upon that one may deny to protoplasm an organisation, in the proper sense of the term, and yet one may consistently attribute to it a structure, even a very complex structure. But that structure need not be called an organisation, to do so is to confuse two clear issues. It is worth while to emphasise this point, for some people think it very inconsistent to affirm that protoplasm has a complex structure and at the same time to deny that it is organised.

I conceive that the view that protoplasm is composed of granules, which are either biophors or secondary aggregates of biophors, has been sufficiently refuted by Butschli's researches on hyaline protoplasm already referred to. The hyaline pseudopodia of Gromia show no trace of granules, not because the granules are too small to be seen, for the highest powers of the microscope reveal in the protoplasm, at the moment of its protrusion to form a pseudopodium, a structure which is not granular, namely, an alveolar structure, and if granules were present they must necessarily be sought for in the alveoli or in the alveolar walls. But they are to be found in neither, so it may be affirmed that in the simplest form of protoplasm there are no granules, a circumstance which deprives the theory of biophors of much

of its weight. Of course it may be objected that the alveolar walls and contents may be composed of biophors so small as to defy detection; such an objection must be defended on theoretical grounds, and I will deal with it presently; just now I will confine myself to the consideration of the visible structure of protoplasm.

After rejecting the granular theory we have a choice of several others; the fibrillar theory, the reticular theory, and the alveolar theory of Bütschli. It would take too long for me to examine these several theories in detail; it has already been done by Bütschli (*loc. cit.*, p. 177), and still more recently by Yves Delage,[1] if I were to undertake the task I should only give a *résumé* of their arguments. For my own part I am strongly inclined in favour of Bütschli's "Wabenlehre".

For some reason or other Bütschli's account of the structure of protoplasm has not, to use a common expression, "caught on". Possibly because it was published at a time when men's minds were occupied with the more alluring prospect offered by the granular theory of protoplasm, with all its delusive hopes of an explanation by means of biophors, and primary organisation of the phenomena of heredity, and of all the vital processes. Possibly also because Bütschli himself pushed the analogy between microscopic foams and protoplasmic structure too far. But if his theoretical considerations are put aside, there is a great deal to be said for his fundamental views. The alveolar structure which he describes may be demonstrated in many various forms of protoplasm. It is particularly obvious in *Pelomyxa*, in which form the larger vacuoles serve admirably as a contrast between the finer alveolar structure which he claims to be common to all protoplasm and the grosser vacuolar structure which is often mistaken for it. I have myself identified the alveolar structure in a considerable variety of protozoa, and in a number of tissue cells, and I have succeeded in making Bütschli's artificial amœbæ, and am

[1] Yves Delage, *La Structure du Protoplasma et les Theories sur l'Hérédité et les grands problems de la Biologie generale.* Paris: C. Reinevald et Cie, 1895.

convinced of the close analogy in structure between the artifact and the natural product. The resemblance between the two is exact, and it is astonishing. The optical characters of the artificial product are explained, on physical grounds, as the outcome of a certain structure, namely, an alveolar structure. The identical optical characters of protoplasm may surely be explained on the same grounds. It is not pushing analogy too far to say that identical optical characters are the result of identity of structure. The analogy is somewhat strained when it is sought to prove that the identity of the streaming movements in the artificial product with those in protoplasm are attributable to the same physical causes. The chemical constitution of the two bodies is so different that the phenomena observed might be regarded as secondary. Nor is the identity absolute, for Bütschli himself points out that the induced currents in the surrounding medium take place in the reverse sense in an amœba to what they do in the case of the microscopic foam. I cannot think that the criticism of O. Hertwig invalidates Bütschli's theory seriously. Hertwig says that lamellæ of oil consist of a fluid which is not miscible with water. If the comparison between the structure of an emulsion and the structure of protoplasm depends on something more than a superficial resemblance, then the lamellæ of plasma which are compared with the lamellæ of oil must consist of a solution of albumen or of a fluid albumen. But a solution of albumen is miscible with water, and therefore it would mix with the contents of the alveoli: emulsions of albumen must be formed with air, not with water. To this Bütschli answered that the framework of plasma consists of a fluid composed of a combination of an albumen and a fatty acid, which was therefore not miscible in water. Another obvious answer is that living plasma is not a simple albuminous solution, for if it were most protozoa could not exist, they would immediately dissolve in the water in which they live. Whether a fatty acid exists in combination with the plasma or not, there is something in the constitution of living plasma which differentiates it from albumen, for it does not

dissolve in water; dead plasma on the other hand becomes albumen and dissolves speedily. What that something is I do not venture to suggest; could we ascertain what it is, no doubt we should have discovered the solution to the riddle of life. Hertwig says that the structural elements of protoplasm, be they filaments, or reticular, or lamellæ, or alveoli, or granules, or what else, have a fixed state of aggregation. Protoplasm is no mixture of two immiscible substances such as water and oil, but consists of a union of fixed organic material particles with abundant water. This is but a verbal statement of the facts and is no explanation, but he adopts later on (*loc. cit.*, p. 49) Nageli's micellar theory as an explanation. No doubt it is the best explanation possible, but it again does not give more than a verbal explanation of the remarkable and fundamental phenomenon that protoplasm, be its structure what it may, does not when alive dissolve in water, but when dead it becomes something else which readily dissolves, provided of course that it is not killed by means which coagulate the albumens into which it is converted at death.

I shall recur again to the micellar theory, for the present purpose it is sufficient to say that it is not inconsistent with Bütschli's "Wabenlehre,"[1] and might even be pressed into service to explain why the plasma does not mix with the watery alveolar contents without the necessity of calling fatty acids to aid.

Supported by these considerations, and by a considerable mass of objective evidence, I venture to think that Bütschli

[1] Bütschli criticises the micellar theory and the analogous theory of "inotagmas" put forth by Engelmann. He does not accept either, but does not give in their place any theory of the ultimate compositions of the substances which form the alveolar framework and contents, except that (p. 309) he says, "a series of reflections . . . led me to suppose . . . that the chemical basis of the framework substance must be formed by a body which has arisen from a combination of albuminoid and fatty acid molecules." Such a combination must mean the formation of a chemical unit of a higher order than the molecules which enter into its composition, and for my purposes such a chemical unit is a micella. In this limited sense the acceptance of a micellar structure is not incongruous with the "Wabenlehre".

has given a true account of the minute structure of protoplasm, so far as it can at present be determined by optical means. And I even venture to prophecy that when the history of the biological work of this half century comes to be written some half century hence, the theories of biophors and plasomes and the such like will have merely a historical interest, whilst the work of Bütschli will be regarded as the most sagacious and far-sighted contribution of our time to this momentous question. In saying this I do not wish to declare my adhesion to the more theoretical part of Bütschlis work, but only to his account of the microscopic structure of protoplasm.

Even if one were to accept his explanation of the streaming movements there would remain all the other phenomena of life to be accounted for, and they are inexplicable on the visible structure of protoplasm, even if it be an alveolar structure.

Underlying the visible structure then there must be an invisible structure, which is the cause of the phenomena. This admission once made, the claims of the rival theories of biophors, plasomes, plastidules and what not, again press themselves on our attention. Now it is to be remarked that the most cautious and thoughtful theorists do not claim that their hypothetical units are an explanation of life. Weismann categorically denies that his theory of the germ plasm is a theory of life, it is only a theory of heredity, but he goes so far as to suggest that a workable explanation of the more complicated vital phenomena may be the surest indication of the path which will lead to an explanation of the more simple (*loc. cit.*, p. 21).

Others, however, are not so cautious, and in any case there is this feature common to all, that they aver on the one hand that vital processes are so complicated that they cannot be explained by a physico-chemical theory of the constitution of protoplasm, and that therefore we must assume the existence of ultimate vital units or biophors: on the other hand, after endowing these biophors with all the attributes of life, they say that they have a comparatively simple molecular constitution upon which the phenomena

which they exhibit depend. In fact they describe essentially similar functions in biophors and in cells, but they allow a physico-chemical explanation in one case and disallow it in the other. This contradiction has been noticed by others, and it has never been satisfactorily explained away. Whitman draws attention to it, and observes that no one, as far as he knows, has looked upon the unit as anything more than the seat of the mystery. This is true, but it is no reason for putting the mystery in a small bag instead of a big one. He defends the theories of smaller units, however, by saying that they have extended our knowledge of organic mechanism (*loc. cit.*, prefatory note, p. vi.). This again I believe to be true, but not quite in the sense in which Whitman apparently means it to be. The theories of minute independent vital units have, I believe, led many on the wrong track as regards vital mechanism; the attacks on such theories are leading to a considerable extension of our knowledge in this direction. The ultimate vital units confessedly do not remove the mystery; ultimately the explanation of life must be a chemico-physical one; there is no alternative but a vitalistic theory, and this is not admissible in science. The strongest ground, *viz.*, the granular hypothesis, for assuming the presence of vital units is removed by the observed constitution of hyaline protoplasm, and finally none of the assumed aggregates of units which are admitted to be visible, are identified with various sorts of granules and considered to constitute units of a higher order, have ever been shown to be capable of leading an independent existence.

On the other hand there is a general consensus of opinion that protoplasm is not a simple organic compound. Its unit is not the molecule, but an aggregate of molecules forming a unit of a higher order to which the molecule stands in the same relation as the atom does to the molecule. It is also admitted that these molecular aggregates may exist in many different kinds in protoplasm. Such a conception is absolutely necessary for the explanation of the most simple properties of organic bodies, for example,

their optical properties and the imbibition of water. But it is a physico-chemical conception, and the molecular aggregate need not and should not be endowed with independent vital powers. Such a molecular aggregate is the micella. In accepting the micella one may attribute any amount of complexity to protoplasmic structure without for a moment admitting that it is a congeries of elementary organisms. Nor need we admit all the theories which Nageli has tried to establish as the necessary consequences of the assumption that there are such things as combinations of polyatomic molecules into groups of a higher order. As I have already said, it was pointed out by von Sachs that even in the region of pure chemistry it is necessary to assume that polyatomic molecules are grouped into closer molecular unions, thus giving rise to chemical properties which did not belong to the individual molecules. But in the region of pure chemistry such a grouping is not called an organisation, nor is there any reason why it should be called an organisation in the present case. Let us be perfectly definite and say that by a micella we mean a combination of polyatomic molecules into closer union to form a group; nothing more, except in so far as we may reason on chemico-physical grounds as to the behaviour of such groups and their relations *inter se.* For instance (I am quoting from O. Hertwig's summary of this part of the micellar theory): "The micellæ exert an attraction both on water and on one another, whereby the phenomena of swelling may be ex-explained. In a dry organic body the micellæ lie close to one another, separated only by exiguous envelopes of water: these latter enlarge considerably during imbibition, since the attractive forces between the micellæ and water are at first greater than between the micellæ themselves. The micellæ are separated from one another by the imbibed water as it were by a wedge; but an organised body does not arrive at a condition of solution, since the attraction of the micellæ for water diminishes in the course of their separation from one another, at a greater rate than the attraction of the micellæ for one another, and therefore, when the watery envelopes have attained a certain size, a condition

of equilibrium, the limit of imbibition is reached." And also: "Since particles of water may be held fast on the surfaces of the micellæ by molecular attraction, so also other matters (lime and siliceous salts, colouring matters, gelatin compounds, etc.) may be deposited on them after they have been taken into the organic body in a state of solution". So far as my physical knowledge enables me to form a judgment, attributes such as these may justifiably be ascribed to micellæ on purely physical grounds and their importance can hardly be overestimated, since the last passage quoted affords a hint as to the nature of the essentially vital process of assimilation. It is not my business now to develop a complete theory; I doubt indeed whether a complete theory is possible in the present state of our knowledge. I have done sufficient for present purposes if I have succeeded in indicating what ideas we may justifiably hold on the subject of protoplasmic structure, and I believe that I have given some good grounds for justification of the views that; (1) the ultimate visible structure of protoplasm is an alveolar structure; (2) that the invisible structure of protoplasm is a "micellar" structure in the sense defined above.

But before I proceed I must enter a caveat against being considered as an adherent of the micellar theory of Nageli. I cannot enter here into my reasons, but I may say that the further theories which Nageli assumes to be the necessary consequences of the existence of micellæ, do not appear to me to be necessary consequences at all; indeed I part company with him at once when I express my conviction that the hypothesis of a micellar structure is compatible with the alveolar structure described by Bütschli.[1]

[1] Since the above argument was first written out the work of Yves Delage has come into my hands. It is most gratifying to find that the opinions of so distinguished an author accord so exactly with my own. The reader who finds my argument involved and laborious may turn with profit to Delage's book, in which he will find a lucidity of expression and a precision in argument which I can only envy without hoping to imitate. It is worth while quoting the following passages here: "On peut accorder

I may now anticipate the objection which is certain to be raised that the visible and invisible structure which I assign to protoplasm is utterly inadequate to explain the phenomena of life. It is inadequate and it is intended to be inadequate. Were I to pretend that it is adequate I should be running counter to all the lessons taught by our experience of living things. The structure which I have assigned to protoplasm applies particularly to that simplest known form of it which we rarely meet with, but which we do meet with in exceptional cases, for instance in the pseudopodia of *Gromia dujardini.* But separate a protoplasmic corpuscle formed by the thickenings of the thread-like pseudopodia of this species from the rest of the animal; the corpuscle separated is not any longer capable of an independent existence, it soon perishes, it has all the structure which I have described, but it is not capable of independent life. Clearly then life is not the outcome of this structure, though the structure may play its part, and no unimportant part in the life processes.

When I have been speaking of protoplasm I have obviously been confining my attention to that form of it which is now generally distinguished under the name of Cytoplasm. Cytoplasm taken by itself is not living matter in the sense that it is capable by itself of maintaining an independent existence. The experiments of Nussbaum,[1] of A. Gruber and Verworn, confirmed by other observers, have

à l'auteur (Nägeli) ses Micelles. Leur constitution, leurs propriétés n'ont rien que de très admissible. Bien que leur mode de génération ne soit guère probable, il n'y a aucune raison positive pour le repousser. Mais l'arrangement des micelles et la structure de l'idioplasma sont invraisemblables au plus haut point. Nous avons démontré, au cours de notre exposé, que cet arrangement n'est pas de tout, comme l'auteur l'avance, le résultat nécessaire du seul jeu des forces moléculaires initiales ce n'est qu'à grand renfort d'hypothèses étagées l'un sur les autres qu'il arrive à faire disposer les Micelles en Files, les Files en Faisceaux, les Faisceaux en Cordons et les Cordons en un Reseau répandu dans tout l'organisme."

[1] It was Nussbaum who first introduced the method of dividing infusoria by artificial means, and the credit of having devised this very useful class of experiment belongs to him. In my previous article I inadvertently assign it to Gruber.

shown that pieces of cytoplasm cut off from the remainder of a protozoon are incapable of maintaining life and soon perish. If, on the other hand, a fragment of cytoplasm similarly cut off contains nuclear matter, it is shown to contain the attributes necessary to life, for the fragment does not perish but reconstitutes itself and becomes an inde-dependent living being. The converse also holds good. A nucleus or a fragment of a nucleus isolated from a protozoon, is incapable of life and perishes. But a nucleus or a fragment of a nucleus in conjunction with a fragment of cytoplasm is capable of life and constitutes an independent living being. The reasonable inference is that cytoplasm plus nuclear matter is indispensable for the performance of vital functions.

Now cytoplasm plus nuclear matter constitutes a cell.

I have elsewhere discussed at some length the definition of a cell,[1] and I have defined it as a corpuscle of protoplasm which contains nuclein. In the present state of our knowledge this definition seems the only one possible. The cell then consists of two essential substances, cytoplasm and a substance which is different from cytoplasm, both structurally and in chemical constitution, namely, nuclein. In a great majority of cells other substances are present which are different from both of these. Such substances are the centrosomes, that modification of cytoplasm which is called archoplasm, amylum and aleurone grains and so forth. As far as we know, however, these substances are not essential to life, but are secondary products characteristic of differentiated cells. Recent researches on the structure of Bacteria and Oscillaria justify the assertion that cells exist in which these substances are absent. We know next to nothing about the presence or absence of centrosomes and archoplasm in the Protozoa, and it may be that further investigation will lead us to the conviction that these two are as essential to the life of these forms as the presence of cytoplasm and nuclein. Maybe not; in any case it does

[1] *Quarterly Journal of Microscopical Science*, vol. xxxviii., p. 137, 1895.

not matter for present purposes. It is sufficient to know that two substances, cytoplasm and nuclein, must be brought together or life cannot exist, and that it does exist in organisms in which these substances, and these only, can be detected, *viz.*, in Bacteria. This statement may appear somewhat hazardous, seeing that the presence of a nucleus is denied in several living beings, in bacteria, for instance, and in yeast. A nucleus in the sense of a centralised body is certainly absent in these and in many other forms, but Bütschli has demonstrated the presence of nuclein in *Oscillaria* in *Bacterium lineola*. As for *Saccharomyces* it undoubtedly contains nuclein, for Raum has prepared it from yeast cells, and the most recent observer, Macallum,[1] is of the opinion that the nuclein is distributed through the cytoplasm but also aggregated in the so-called granules of Raum.

The statement therefore can scarcely be called hazardous, and it is really warranted by the facts at our disposal, for the more carefully that researches are made, and the more delicate the methods of investigations employed, the more is the presence of nuclein demonstrated where it was not previously supposed to exist.

Macallum's paper, by the way, is of great interest, for he shows that nuclein is essentially the iron-holding substance in cells. Knowing as we do the close connection there is between the presence of iron and the due performance of the vital processes, this observation opens up a fruitful source of inquiry as to the dependence of life on chemical processes.

Throughout this argument I have tried to stick to the rule of drawing legitimate inferences from observed facts without wandering into the obscure regions of hypothesis. If I have been successful and have fairly stated the facts, and have drawn legitimate inferences, the conclusion which I come to must be admitted to be of considerable weight.

[1] A. B. Macallum, "On the distribution of Assimilated Iron Compounds, other than Hæmoglobin and Hæmatins, in Animal and Vegetable Cells," *Quart. Jour. Mic. Sci.*, vol. xxxviii., pp. 175-274, 1895.

The conclusion is this: *that life is possible only when two (or more) substances of complex chemical constitution are brought together, and that when these two (or more) substances are brought together we have before us a cell. The cell therefore is the vital unit* κατ' ἐξοχήν. *The component parts of the cell are not vital units, for by themselves they are incapable of life; they are the auxiliaries, the indispensable auxiliaries of life, but they are not themselves living.*

This is not a theory of life, and it does not pretend to be one. It is the generalisation which the facts seem to warrant, and if it be true, as I believe it must be true, it is entirely inconsistent with the whole group of theories based upon hypothetical biophors, gemmules, plasomes, physiological units, plastidules *et hoc genus omne*. Those theories are false. And the cell theory is not inadequate, but it is the only theory which our knowledge of structure and of life processes permits us to adopt, at least if we confine ourselves to that part of it which is essential, namely, that there is one general principle for the formation all tissues, animal and vegetable, and that principle is the formation of cells.

Cells are the ultimate vital units, though they are not the ultimate structural units; *they* are the Lebensträger, or biophors, and there are no living individuals lower than cells.

As I have made an effort to stick to facts and have slighted hypotheses, I shall doubtless incur the profound contempt of those superior persons who find no mental repose in things which can be clearly apprehended, but must leave the material support of earth and seek for rest on the unsubstantial pillows of cloudland. They will have abundant scope for exercising their contempt, for my conclusion explains nothing, and gives no clue to the problems of heredity.

As I have said in the earlier part of this essay, I have no intention to discuss here the complicated problems which are involved in the question of heredity. I take my stand on the position from which I started, namely, that if minute

vital elements occur at all, those same elements which make life possible and control assimilation and growth must also be the agents in bringing about the phenomena of heredity. I have shown that minute vital elements smaller than cells cannot be believed to exist, and it is clear that the phenomena of heredity cannot be explained by things which have no existence. This is a sufficient answer to those who would say that the phenomena of heredity are such that we must make use of a hypothesis of minute vital elements, which are at once the bearers of the vital qualities and the bearers of the heritable qualities (the historic properties if the expression is preferred) of protoplasm. It is not true that a theory of heredity is impossible unless such elements are postulated. Delage has brought forward a theory of heredity which discards altogether the use of hypothetical biophors. I pass no criticism on his theory, favourable or unfavourable, but call attention to it merely for the purpose of showing that a theory without biophors is possible. It is no argument to say that the theories based on ultimate vital units have largely extended our knowledge of heredity. The Ptolemaic system of astronomy largely extended men's knowledge of the movements of the heavenly bodies, but it was not on that account a true theory.

Moreover, it will be hardly fair to twit me with the fact that I renounce, for the present, an attempt to explain the most complicated manifestations of life, for this is only an essay, and makes no pretence to be the development of a doctrine.

It is not my present intention to frame hypotheses, not because I undervalue the use of hypothesis, but because I regard the first necessary step to be the formation of ideas appropriate to the facts.

Dr. Whitman has recently written quite a nice little lecture on the subject of fact and theory, and has directed it against myself in particular, winding up with a trenchant paragraph to the effect that the claim to a monopoly of fact reflects an arrogance which seems to be epidemic. This homily is fortified by quotations from von Baer,

Goethe, Huxley and Whewell. Now I never claimed a monopoly of fact, but that facts should receive a due share of recognition. Mutual service, as Whitman says, is the principle which ties theory and fact together; quite so, but when theory runs altogether away from fact, the mutual service is wanting. Fact is a slow servitor, and drags heavily on the impatient feet of theory. The quotations from Goethe and Huxley do not lend support to the practice of making hypotheses, rather the contrary. "Experience, Reflection, Inference" is an excellent motto, but inference does not mean making hypotheses, nor yet does the necessary process of generalisation and classification which Huxley recommends. The passage quoted from the last-named author condemns the mere cataloguing of facts under the name of Science, but it does not countenance the reckless use of theory. As for Whewell's aphorism, let me commend to Whitman a study of what that author says with regard to the failure of the Greek schools of philosophy. They did not fail, he says, because they neglected facts; the Aristotelian school may be held to have surpassed the moderns in its appreciation of the value of facts. The Greeks certainly did not fail for want of boldness in theorising, nor for want of acuteness, of ingenuity and power of close and distinct reasoning. Nevertheless with all help from the twin-service of fact and theory their philosophy was a failure, and why? Because, as Whewell points out, their ideas were not distinct and appropriate to the facts. May not the same thing be said of many of the theories of cell life and of heredity which have been so much in vogue in the last few years? It was my object when I wrote on Epigenesis and Evolution to show that some ideas then current, were not appropriate to the facts; it has been my object in the present essay to show that certain theories on cell life, beautifully constructed and ingeniously defended as they have been, are not appropriate to the facts. I am far from undervaluing the use of theory, and when I took occasion before, as I have done again now, to emphasise the importance of attention to fact, I was not quite so ignorant nor so arrogant as Whitman supposed. The motto of Goethe

might well have been reversed for the adornment of the title pages of some works of the last twenty years. "Theory, reflection, experience," the last named to be fitted in as best it might.

Since the above passages were first written the great work of Yves Delage has came into my hands. Mine is not the only voice crying out in the great wilderness of theories. This new voice, however, is far greater and more powerful than mine. The reader who may be unconvinced by my clumsy argumentation should turn to the pages of Delage. For clear and candid exposition, trenchant criticism, and rigorous exposure of defects of reasoning, they are unsurpassed. Now that this part of my work is ended I feel that it need never have been begun, for all that I have had to say has been said in greater detail and with much greater force by Delage.

# FERNS, APOSPOROUS AND APOGAMOUS.

THE normal life cycle of ferns, owing to the microscopic character of their reproductive apparatus, long baffled the comprehension of botanists. But some half a century ago, starting with the observations of Naegeli and Suminski and culminating in those of Hofmeister, the whole course of their ontogeny has been cleared up. The fern plant, as ordinarily so-called, produces on the back of its leaves or fronds, countless numbers of spores, which are formed within minute capsules or sporangia. When these spores germinate they give rise, not to a new fern plant, but to a leaf-like scale—the Prothallus. Upon the lower surface of this the sexual organs arise, and within them the sexual cells themselves are differentiated, and as the result of the fertilisation of one of the female cells or oospheres, by the male cell or antherozoid, a new fern plant arises. Thus in normal cases a regular alternation of a sexual with a sexless generation is seen. But although this is the course followed by the vast majority of the ferns which have been hitherto investigated, it is not the only one open to the plants. Thus Prof. Farlow in 1874 discovered that the formation of the sporophore (fern plant) generation might arise directly from the oophore (prothallus) generation, without the intervention of sexual organs, by a process resembling ordinary budding. De Bary, who followed the matter further, found that several ferns other than that examined by Farlow reproduced themselves in the same fashion, to which phenomenon the name of Apogamy was given, the marriage link being eliminated. Curiously enough De Bary found that a variety of one of our most vigorous British ferns reproduced itself constantly in this asexual manner, though the common form exhibited no abnormality in this respect. Recently, however, L. Kny,[1] pursuing these investigations further, has found the

[1] *Entwickelung von Aspidium Filix mas. Sw.*, 1 Theil., L. Kny, Berlin.

normal form to reproduce itself in both ways, and since his asexual examples occurred in thickly-sown pots, it would appear to be due to some extent to a starved condition induced by overcrowding, which checks the formation of the archegonia, and leads to the simple budding in their place. In all these instances the young plants are engendered upon precisely the same spots on the prothallus as the sexual one would occupy, and as their development and appearance are identical, it is only by preliminary watching that their apogamic origin can be determined.

A case of Apogamy (or rather two cases), however, recently occurred in a sowing of my own, which is quite distinct from any I have seen described. A sowing of a plumose variety of *Athyrium filix fœmina* failed almost entirely, only two or three prothalli surviving. One of these after growing very large, nearly half an inch across, remained perfectly dormant the whole of the summer; early in the autumn, however, the edge of the prothallus began to grow out and upwards in two places, and eventually two slightly curved horns,[1] each about one quarter inch long, developed perpendicularly, one on each side of the indentation or sinus common to most prothalli. Later on, at a short distance from each tip, a small whitish bulbil appeared and these increased in size until the circination of several fronds was plainly visible, a small crown or caudex being developed. No roots, however, were emitted, and the two little plants, both, be it remarked, identically situated and very like in form, were evidently supported by the prothallic root-hairs, though by this time most of the prothallus was brown and dead. Subsequently I placed a piece of loam in contact, and into this both plants rooted and fronds were sent up, the first of which had no less than ten pinnate divisions on either side. It was thus, it will be seen, very different from the usually simple primary fronds produced either sexually or apogamously heretofore. Later on still, noticing that the tips of the horns were showing signs of dilating, I cut these off with a razor and laid them

[1] *Gard. Chronicle*, 10th Nov., 1894.

down, two apparently normal and full-sized prothalli being the present result. In this case it will be noted that both plants were far removed from the usual site of reproduction, and both in this respect and in their vigorous development are differentiated from previously cited cases of apogamy. The second case alluded to occurred on another prothallus in the same pan, wherein the bulbil developed likewise upon a horn-like excrescence, but on the centre of the upper surface of the prothallus. This bulbil has developed into what is so far a very weakly plant of a different type to the others, but otherwise presenting no special feature.

Until 1884 the Prothallus had always been regarded as necessarily the offspring of the spore, but in the autumn of 1883 a presumed barren variety of *Athyrium filix fœmina* (*var. Clarissima*) was sent me for examination. For twenty years this plant had been observed to produce an immense number of apparent sori, but no spores were ripened or shed, and no offspring had consequently been raised. Some previous observations on dorsal bulbils, *i.e.*, bulbils associated with the spore heaps in this same species, led me to the opinion that these apparent sori, which consisted of green pear-shaped masses instead of the capsules proper to spores, did not represent bulbils, but some abnormality in the development of the sporangia. To test this I laid down portions of the fronds, and to my intense surprise these pearshaped bodies commenced at once to grow into prothalli, their tips dilating and spreading, while root-hairs and subsequently both archegonia and antheridia appeared in abundance. I at once gave a note of my observations at the Linnean Society[1] as demonstrating the development of the prothallus without the agency of the spore. The following season, pursuing my culture, I was able to exhibit a number of plants and such material as satisfied the society of the facts put forward.[2] Prof. F. O. Bower[3] then undertook

[1] "Observations on a Singular Mode of Development in the Lady Fern (*Athyrium filix fœmina*)," *Linn. Soc. Journal Botany*, vol xxi., p. 354-7.

[2] "Further notes on ditto," *ibid.*, vol. xxi., pp. 358-60.

[3] "On Apospory in Ferns (with special reference to Mr. Charles T. Druery's observations)," F. O. Bower, *ibid.*, vol. xxi., pp. 360-68.

the further investigation of the case, and found that the development of the sorus or spore heap went as far as the formation of the stalk of the sporangium or spore capsule, but at that stage it stopped and a vegetative growth set in to form the clusters of pear or club-shaped bodies which eventually went through the normal evolution of prothalli and sexual plants. Mr. G. B. Wollaston followed by providing material from a variety of *Polystichum angulare* in his possession, wherein the elimination of the spore and the entire soral apparatus was so complete that the prothalli were developed from the slender-pointed tips of the ultimate divisions of the fern-frond. Padley, *P. ang. var pulcherrimum* was the plant in question, and as it chanced that several other varieties of the same type existed, though found at widely sundered spots in England, it resulted that Dr. F. W. Stansfield and myself found the same character in two of them. Prof. Bower further observed that soral apospory, *i.e.*, the form first noted, was also present on Padley's plants, and this too we, Dr. Stansfield and myself, confirmed in the others. We have in these four examples, and in the genus Polystichum especially, ample proof that the spore is not an essential preliminary to the existence of the Prothallus, but that the latter may be developed direct from the tissues of the Sporophore, precisely as this latter in Apogamy may be developed direct from those of the oophore.[1] Curiously enough the next case which came before the writer's notice was an aposporous seedling of the same variety of Lastrea (Aspidium) determined by De Bary as being persistently apogamous, *viz.*, *Lastrea pseudo mas var. cristata.* This case was distinct from previous ones as it was a young plant and not an adult, which produced the prothalli. The tip of the second frond evolved from the prothallus (the first was eaten off and was not seen) bore a prothallus of the normal form. Subsequently this and the succeeding

[1] Professor F. O. Bower subsequently prepared an exhaustive monograph "On Apospory and Allied Phenomena". *Linnean Transactions*, vol. ii., part 14, July, 1887, to which reference should be made for details of the preceding cases.

frond became covered with prothalli developed not merely from the edges, but also from the upper surface, and being pegged down produced a number of plants, but whether apogamously or not I cannot say, though from De Bary's observations, they should be so. It is worthy of remark that in some of these youngsters, the line between the two generations of sporophore and oophore was so vague that the primary fronds were simply stalked prothalli, the next frond half one and half the other, while the fourth or fifth had quite outgrown the tendency and were of the typical varietal form. This plant was exhibited and described at the Linnean Society, 3rd November, 1892.[1] Of the next two cases I observed, the first was an Athyrium found in Lancashire and exhibited in 1893 at the meeting of the Pteridological Society at Lancaster by Mr. Bolton the finder. Immediately on seeing it I remarked, "How very like Col. Jones's Clarissima," simultaneously with which Mr. Bolton said, "It is strange, but it never ripens its spores"". Turning the frond over, the reason was clear, it was perfectly white with aposporal excrescences. On submitting these to culture they produce plants freely by sexual action, but of two types, one very depauperate, mere skeleton plants, and the other of the parental form with occasional reversion towards the normal. In some of these young plants the whitish excrescences are plentiful in fronds only an inch or two high, and there are evident signs of prothalloid growth at the tips of the segments as well, pointing to apical apospory when the plants are more developed. The next case occurs in a most unlikely species, especially as apical apospory is in question. This is seen in a variety of *Scolopendrium vulgare* (*S.* v. *crispum Drummondiæ*) which occurs in the wild state, like all the rest, characterised by being frilled and crested, and having moreover a finely fimbriated edge to the fronds. Visiting Mr. Bolton to inspect the Athyrium last cited, I saw a fine plant of this fern, and it immediately struck me that the tips of

[1] "Notes on an Aposporous Lastrea (Nephrodium)" *Linn. Soc. Journal Botany*, vol. XXIX., pp. 479-82.

the fimbriate projections were remarkably translucent. I obtained material, laid it down, and at once prothalli began to develop vigorously from every point, so vigorously indeed that a single tip has formed a mass of prothalli an inch across which yielded at least a dozen plants of the parental form.[1]

Dr. F. W. Stansfield has recently sent me prothalli developed from a finely fimbriated form of Lastrea of which the reputed parent is that already described, and informs me that it is profusely aposporous though fairly developed in size.

By the various instances of this phenomenon so far cited, we see that the normal life cycles of the ferns in question have been successively shortened, first by the elision of the spore and then by that of the whole soral apparatus, while if we accept De Bary's observations as establishing the constant apogamous reproduction of *L. pseudo mas cristata*, in that case, it is shortened almost to the utmost, the chain being simply sporophore, prothallus, sporophore. Consistently indeed with the alternation of generation the chain could not apparently be shorter since the prothallus being eliminated we naturally come, or seem to come, to simple bulbils, such as occur on many ferns, *Asplenium bulbiferum* for example. In the final case, however, which I have to cite, we arrive at the elimination even of the prothallus by substitution of the frond itself as the oophore or egg-bearer, the archegonia and antheridia being generated upon the frond without the prior formation of a prothallus proper. In a small plant of *Scolopendrium vulgare* recently sent me by Mr. E. J. Lowe, and exhibited by me at the Linnean Society in November last, although a definite axis of growth had been formed and several fronds had arisen in the normal spiral fashion around it, indicating that the prothallus stage had been unmistakeably passed, each of these fronds bore a thickened cushion at its tip upon which were seated both

[1] "Note on Apospory in a form of *Scolopendrium vulgare*," etc., *Linn. Soc. Journal*, vol. xxx., pp. 281-84.

antheridia and archegonia, accompanied by aerial roothairs, the frond itself thus assuming the functions of the prothallus. Mr. Lowe raised a number of similar plants on the genesis of which he is preparing a paper which I will not forestall; but he informs me that in time they throw off this aposporous character. Fronds which he has sent me, and which I have laid down, have developed prothalli all over their surface and at all terminals, but so far my cultures are too recent to permit me to report the advent of plants.

This completes the sketch of the cases which have come under my immediate notice, but considering that, including the first discovery, the phenomenon has been observed in no less than nine instances in our limited number of British species, *viz.*, *Lastrea* (*Nephrodium*) two, *Athyrium filix fœmina* two, *Polystichum angulare* three, and *Scolopendrium vulgare* two; it is only reasonable to expect that many undiscovered instances must occur in the innumerable other species existent throughout the world.

CHARLES T. DRUERY.

# Science Progress.

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## THE GROWTH OF OUR KNOWLEDGE OF HELIUM.

### THE DISCOVERY OF THE LINE $D^3$, 1868.

IN the year 1868, spectrum analysis was first utilised in endeavouring to unravel the message which was conveyed to us by a most interesting eclipse observed in India. The diagrams will indicate the kind of record with which we have to deal in studying these celestial hieroglyphics. We are in one part dealing with the long waves of light, the red; we are in the other dealing with the shorter waves of light, the blue. The work done in that eclipse is indicated by the bright lines—the hieroglyphics—which, when translated as they have been, describe for us the chemical nature of the particular stuff in the sun, which made him put on a blood-red appearance "on his getting out of his eclipse". Taking the notes in the light scale which are lettered in the ordinary spectrum of sunlight, in order that they may be easily recognised and remembered, we learn the particular qualities of the light emitted by the blood-red streak.

We have one quality represented by the line D, another at C, and another at F. According to the diagram, one of the lines is in the position of D. One observer said it was "at D, or near D".

Soon after this eclipse was observed in India, a method,

long before formulated, of studying the blood-red streak surrounding the sun without waiting for an eclipse was brought into operation.

By this method it was quite easy to make observations whenever the sun was shining, perfectly free from any of the difficulties attending the hurry and the worry and the excitement of an eclipse, which lasts only a few seconds.

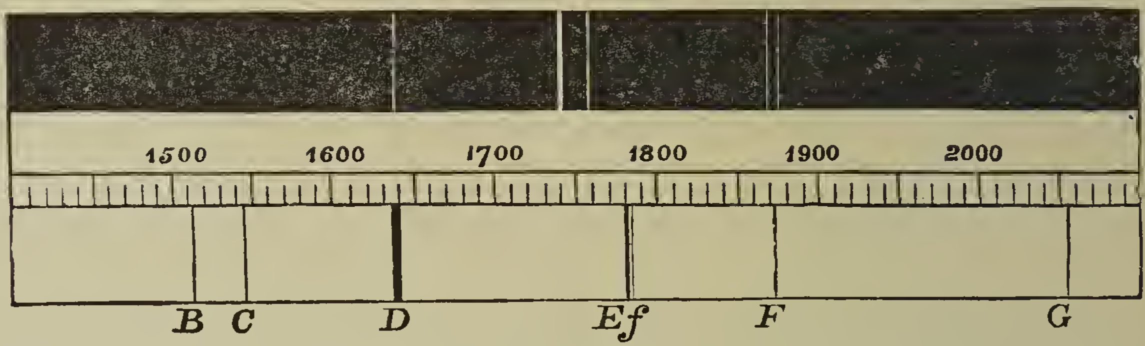



FIG. 1.—Pogson's diagram of the spectra of the sun's surroundings in the Eclipse of 1868. The bright lines seen are shown in the upper part of the diagram ; the chief lines in the solar spectrum, red to the left, blue to the right, are shown in the lower part.

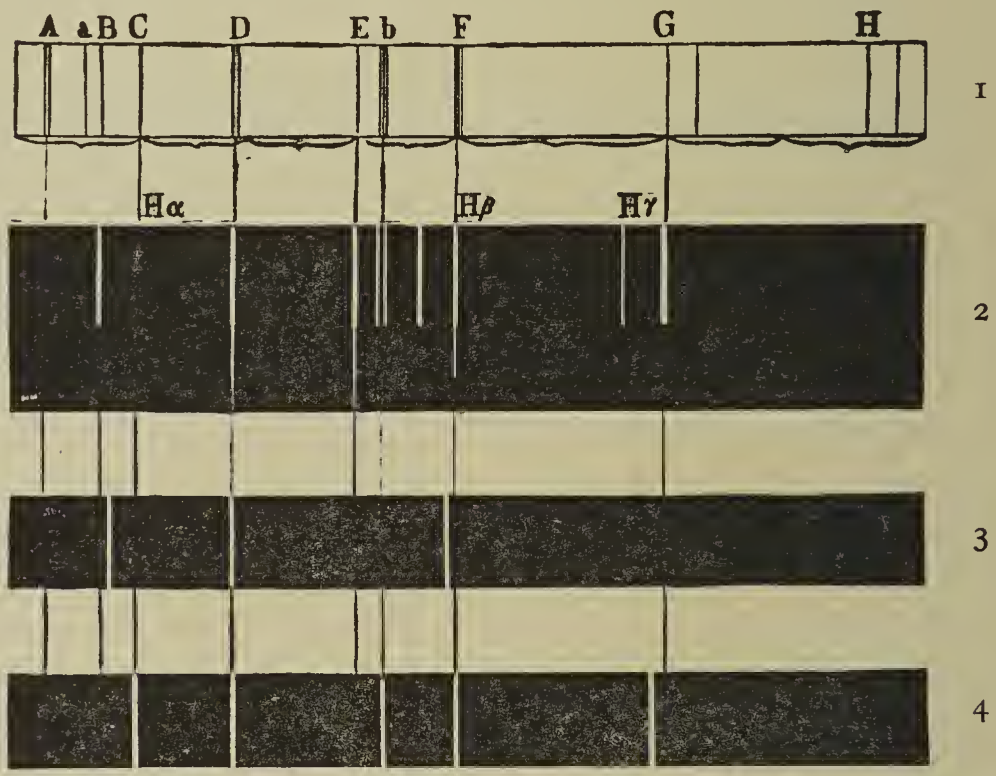
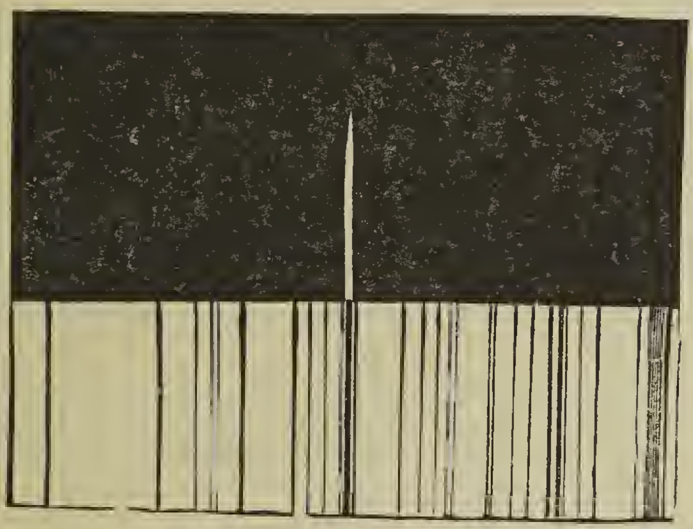



FIG. 2.—Summation of the observations of the spectrum of the sun's surroundings in the Eclipse of 1868. (1) Solar spectrum showing the position of the chief lines. (2) Rayet's observations of bright lines. (3) Herschel's observations of bright lines. (4) Tennant's.

Further, as the method consists of throwing an image of the sun, formed by a telescope, on to the slit of a spectroscope, so that the spectrum of the sun's edge and of the sun's surroundings can be seen at the same time, exact coincidence or want of coincidence between the bright and dark lines can be at once determined. During an eclipse

this of course is not possible, as the ordinary spectrum of the sun, with its tell-tale dark lines, is invisible because the sun, as we ordinarily see it, is hidden by the moon.

Working, then, under such very favourable conditions, it was seen that there was certainly a red line given by this lower



C

FIG. 3.—The exact coincidence of the red line with the dark line C.

part of the solar atmosphere coincident with the very important line in the solar spectrum which we call C.

Another part of the spectrum in the blue-green was examined, and there again it was seen that the parts outside the sun gave us a bright line exactly in the position of

F

FIG. 4.—The exact coincidence of the blue-green line with the dark line F.

the obvious dark line in the solar spectrum which is called F; so that with regard to those two most important lines, there was no doubt whatever that we were dealing with the substance which produces these dark lines in the solar spectrum.

Fig. 5 is a diagram of the yellow, or rather the orange, part of the solar spectrum, showing two very important lines, which are called the lines D, due to the metal sodium, the investigation of which was just as important in solving the celestial hieroglyphics we call spectral lines as the Rosetta stone was important in settling the question of the Egyptian ones.

Pogson, in referring to the eclipse of 1868, said that the orange line was "at D, or near D". We see from the

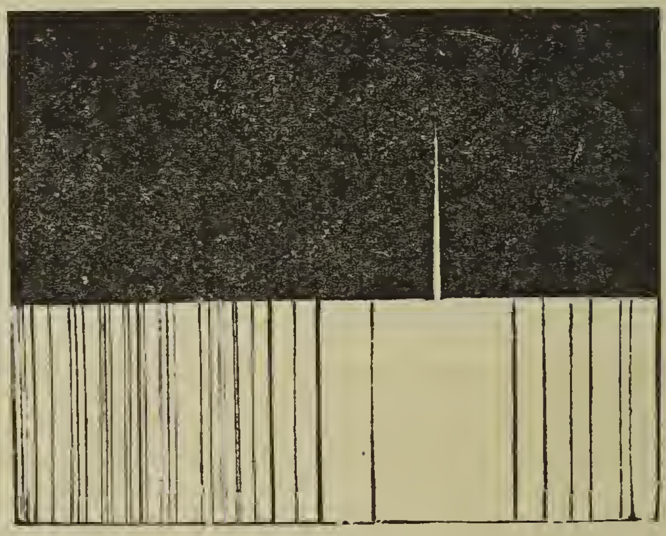



FIG. 5.—The want of coincidence of the orange line $D^3$ with the dark lines $D^1$ and $D^2$.

diagram (Fig. 5) that the new method indicated that "near D" was the true definition. The line in this position in the spectrum, unlike the other two lines which I have indicated, has no connection at all with any of the dark lines in the ordinary solar spectrum. We were therefore perfectly justified in attaching considerable importance to this divergence in the behaviour of this line, taking the normal behaviour to be represented by the two strong lines in the red and the blue-green. The new line was called $D^3$ to distinguish it from the sodium lines $D^1$ and $D^2$.

A considerable amount of work was done with regard to the orange line. It was found that there was no substance in our laboratories which could produce it for us, whereas in the case of the line D we simply had to burn some sodium, or even common salt, in a flame to produce it, and the other lines in the red and the blue-green were easily made manifest by just enclosing hydrogen in a vacuum tube, and passing an electric current through it,

or observing the spectrum of a spark in a stream of coal-gas.

Now at the first blush it looked very much as if this line was really due to the same element which produced the others at C and F, and it was imagined that the reason we did not see it in our laboratories was because it was a line which required a very considerable thickness of hydrogen to render it visible. That was the first idea, and Dr. Frankland and myself found that there was very considerable justification for this view, because a simple calculation showed that the thickness of the solar atmosphere, which was producing that orange line under the conditions which enabled us to see it in our instruments by looking along the edge of the sun, was something like 200,000 miles.

A B C

FIG. 6.—Changes of wave-length of the F hydrogen line when a solar cyclone is observed. A, the change towards the red indicates the retreating side of cyclone. C, the change towards the blue indicates the advancing side. B, the whole cyclone is included in the width of the slit, and both changes of wave-length are visible.

Hence, in order to get a final decision on this point, there was nothing for it but to tackle the question from a perfectly different point of view, and the different point of view was this. The work had not gone on very long before one found minute alterations in the positions of these lines in the spectrum; the orange line, for instance, might sometimes be slightly on one side, and sometimes on the other of its normal position. Further work showed that in these so-called "changes of wave-length" we had a precious means of determining the rate of movement of the gases and vapours in the solar atmosphere.

Fig. 6 indicates how these changes of wave-lengths are

shown in the spectroscope. The lines are contorted in both directions, and sometimes to a very considerable extent, indicating wind movements on the sun, reaching and sometimes exceeding 100 miles *a second.*

We had here a means of determining whether the orange line was produced by the same gases which gave the red and blue lines, because if so, when we got any alteration in the position of the red and blue lines, which always worked together, we should get an equivalent alteration in the position of the orange one.

I found that the orange line behaved quite differently from either the red or the blue lines; so then we knew that we were not dealing with hydrogen; hence we had to do with an element which we could not get in our laboratories, and therefore I took upon myself the responsibility of coining the word helium, in the first instance for laboratory use.

This kind of work went on for a considerable time, and what one found was, that very often in solar disturbances we certainly were dealing with some of the lines of substances with which we are familiar on this earth; but at the same time it was very remarkable that when the records came to be examined, as they ultimately were with infinite care and skill, it was found that not only did we get this line in the orange indicating an unknown element associated with substances very well known, like magnesium, but that there were many other unknown lines as well. Within a few months of my first observations, several new lines about which nothing was known were thus observed.

### THE DISCOVERY OF OTHER UNKNOWN LINES, 1869.

The place of the orange line $D^3$ I determined on 20th October, 1868. Among many other lines behaving like it, two at wave-lengths 4923 and 5017 were discovered in June, 1869, and afterwards another at 6677, while Professor Young noted another in September, 1869, at 4471. He wrote:—

"I desire to call special attention to 2581·5 [=4471 on

Kirchhoff's scale], the only one of my list, by the way, which is not given on Mr. Lockyer's. This line, which was conspicuous at the Eclipse of 1869, seems to be *always present* in the spectrum of the chromosphere. . . . It has no corresponding dark line in the ordinary solar spectrum, and not improbably may be due to the same substance that produces $D^3$."

This same line was noted also by Lorenzoni and named *f.* Another line at 4026 was added later by Professor Young.

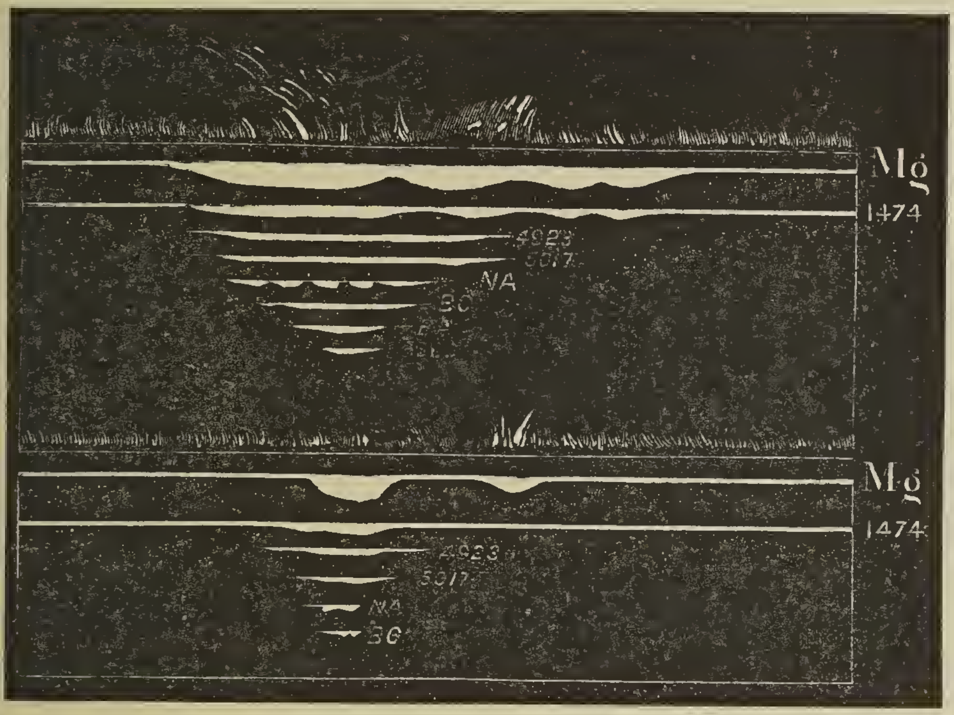



FIG. 7.—Tacchini's observations of two slight solar disturbances showing the height to which the layers of the different gases extend. Magnesium vapour is highest of all, and is furthest extended; next comes a gas of still unknown origin, indicated by a line at 1474 of Kirchhoff's scale and so on.

Then with regard to solar disturbances. Let me refer in detail to a diagram indicating some results arrived at by the Italian observers. We are dealing with the spectroscopic record of two slight disturbances in a particular part of the sun's atmosphere. The spectroscope tells us that in that region there was a quantity of the vapour of magnesium which is collected in that place. Then we find that another substance, about which we again know nothing whatever, is also visible in that region, and then we get the further fact that in those particular disturbances we get four other spectral lines indicated as being disturbed, and of those four lines we only know about one.

In that way it very soon became perfectly clear to those who were working at the sun, that in all these disturbances, or at all events in most of them, we were dealing to a large extent with lines not seen in our laboratories when dealing with terrestrial substances ; this work went on till ultimately, thanks to the labours of Professor Young in America, we had a considerable list of lines coming from known and unknown substances which had been observed under these conditions in solar disturbances, and Professor Young was enabled to indicate the relative number of times these lines were visible. For instance, the lines which are most frequently seen under these conditions he tabulated as represented by the number 100, and of course the line which was least frequently seen would be represented by 1 ; and therefore from these so-called "frequencies" we got a good idea of the number of times we might expect to see any of these disturbance-lines when anything was going on in the sun.

It was this kind of work which made Tennyson write those very beautiful lines :

> "Science reaches forth her arms
> To feel from world to world".[1]

[1] And then he added :

> "and charms
> Her secret from the latest moon".

I mention this because Tennyson, whose mind was saturated with astronomy, had already grasped the fact that what had already been done was a small matter compared with what the spectroscope could do ; and now the prophecy is already fulfilled, for by means of the spectroscopic examination of the light from the stars we can tell that some of them are double stars, that is to say, in poetic language, stars with attendant moons. Although we can thus charm the secret from each moon by means of the spectroscope, to see the moon it would require (in the case of $\beta$ Aurigæ) a telescope not eighty feet long, but with an object-glass eighty feet in diameter, because the closer two stars are together the greater must be the diameter of the object-glass, independently of its focal-length and magnifying power.

## DR. HILLEBRAND'S RESEARCHES ON URANINITE, 1888.

In this year Dr. Hillebrand, one of the officials in the Geological Department at Washington, was engaged upon the chemical examination of specimens of the mineral uraninite from various localities.

He dealt with crystals which he put in a vessel containing some sulphuric acid and water.. He found that bubbles of gas were produced out of the crystal by means of the sulphuric acid. He collected this gas and came to the conclusion that it was nitrogen.

This result was new. He thus wrote about it :—

"In consequence of a certain observation" [the one I have just referred to] "and its results, an entirely new direction was given to the work, and its scope wonderfully broadened. This was the discovery of a hitherto unsuspected element in uraninite, existing in a form of combination not before observed in the mineral world."

It is not needful here to follow Dr. Hillebrand through all the painstaking and patient labour he cut out for himself to explain this anomalous behaviour. Needless to say he did not omit to employ the spectroscope to test the nature of the new gas.

His observations were thus described :—[1]

"In a Geissler tube under a pressure of ten millimetres and less, the gas afforded the fluted spectrum of pure nitrogen as brilliantly and as completely as was done by a purchased nitrogen tube. In order that no possibility of error might exist, the tube was then reopened and repeatedly filled with hydrogen, and evacuated till only the hydrogen lines were visible. When now filled with the gas and again evacuated, the nitrogen spectrum appeared as brilliantly as before, with the three bright hydrogen lines added."

On this paragraph I may remark that it has long been known that gases like nitrogen give us quite distinct spectra at different temperatures—one fluted, another containing

[1] "On the Occurrence of Nitrogen in Uraninite," *Bulletin*, No. 78, U.S. Geol. Survey, 1889-90, p. 55.

lines. Which of these we shall see in a tube will depend upon the pressure of the gas and the electric current used. The fluted spectrum of nitrogen is very bright and full of beautiful detail in the orange part of the spectrum; the line spectrum, on the other hand, is almost bare in that region.

It is important to note *that it so happened* that the pressure and electric conditions employed by Dr. Hillebrand enabled him generally to see the fluted spectrum. This however was not always the case. In an interesting letter to Professor Ramsay he writes (*Proc. Roy. Soc.*, vol. lviii., p. 81):—

"Both Dr. Hallock and I observed numerous bright lines on one or two occasions, some of which apparently could be accounted for by known elements—as mercury, or sulphur from sulphuric acid; but there were others which I could not identify with any mapped lines. The well-known variability in the spectra of some substances under varying conditions of current and degree of evacuation of the tube led me to ascribe similar causes for these anomalous appearances, and to reject the suggestion made by one of us in a doubtfully serious spirit, that a new element might be in question."

Dr. Hillebrand concludes his paper as follows:—

"The interest in the matter is not confined merely to a solution of the composition of this one mineral; it is broader than that, and the question arises: May not nitrogen be a constituent of other species in a form hitherto unsuspected and unrecognisable by our ordinary chemical manipulations? And, if so, other problems are suggested which it is not now in order to discuss."

### $D^3$ AND OTHER UNKNOWN LINES IN NEBULÆ, 1890.

A negative of the nebula of Orion, taken at my observatory at Westgate-on-Sea in 1890, contains fifty-six lines, and of course by determining, as we have been able to do approximately, the wave-lengths—the positions of these lines in the spectrum—we can determine the exact light notes represented, and therefore the substances which produce them. In this spectrum of the nebula of the Orion were

lines of unknown origin exactly coinciding with those unknown lines which I have already referred to as having been seen in the sun's atmosphere. Some of the unknown lines in that atmosphere, those that we have not been able to see in our laboratories, are identical in position with some of the unknown lines in the nebula of Orion.

I may remark that as early as 1886 Dr. Copeland had discovered $D^3$ in the visible spectrum of the nebula, and in a letter to him I had suggested that another line he had recorded at 447 might be Lorenzoni's *f*; this he thought to be probable. The matter was set for ever at rest by the photograph which established the presence of 4471 and 4026 as well, already noted as a solar line.

Professor Campbell, of the Lick Observatory, obtained other photographs of the spectrum of the nebula some two or three years after mine was taken. In the following list of lines in my photograph an asterisk denotes that Campbell gives a line nearly in the same position. He recorded no line which did not appear on my photograph.

3896*
3888*
4011
4026*
4121*
4143*
4168
4390*
4472*
4716*
4924

$5875{\cdot}8 = D^3$

## THE SAME UNKNOWN LINES OCCUR IN THE STARS, 1892.

About the year 1890 I began the photography of stellar spectra at Kensington, with special reference to their classification on the basis of the chemical constituents established by their spectra. By 1892 several important results had been obtained, while the progress of this branch of science lately has been so considerable that any statement regarding the positions of lines, and therefore the

chemical origins of them, may be made with a considerable amount of certainty as depending upon very accurate work.

The various classes in which the stars have been classified by different observers according to their spectra are discussed elsewhere, but some of the more salient differences must be pointed out here; thus we have stars with many lines in their spectra, others with comparatively few. I will take the many-lined stars first.

The diagram (Fig. 8) represents the spectrum of Arcturus, a star the spectrum of which closely resembles that of the sun. In $\alpha$ Cygni we have another star with many lines, but here we note, when we leave the hydrogen on one side and deal with the other stronger lines, that there is little relation between the solar spectrum and these lines.

I next come to the stars with few lines: these are well represented by many of the chief stars in the Constellation of Orion. Bellatrix is given as an example (Fig. 9).

Then, I have next to say that in the photographs of the spectra of many stars, chiefly of those more or less like Bellatrix, we found the same lines which we have so far classified as unknown for the reason that in our laboratories we have not been able to get any lines which correspond with them. I again mention $D^3$, 4471 and 4026, previously noted as appearing both in the chromosphere and in the nebula of Orion.

But the thing is much more interesting even than this; not only these, but all the chief unknown lines appearing in the nebula of Orion are also found in these stars. And this is so absolutely true that there is no necessity to give a list of the unknown lines seen in Bellatrix; every one of them given in the nebula has found its place, and (so far) practically no others.

This of course marked a great development of the inquiry, and makes the question of the unknown lines more important than ever.

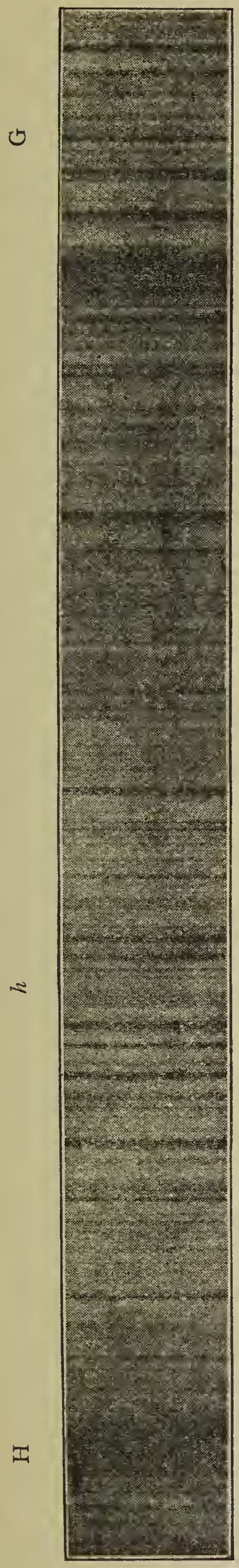



FIG. 8.—Spectrum of Arcturus between G and K.

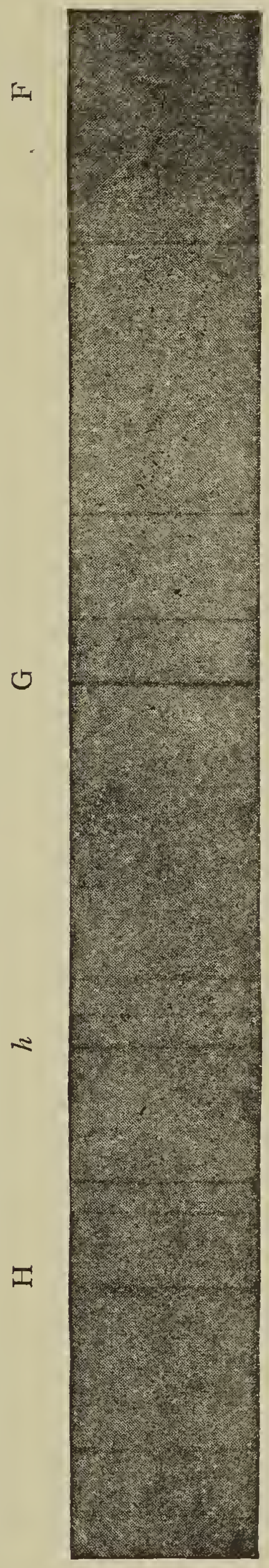



FIG. 9.—The spectrum of Bellatrix between H and F.

PHOTOGRAPHIC RESULTS DURING A SOLAR ECLIPSE, 1893.

A method which was first employed by Respighi and myself during the eclipse of 1871, was employed on a large scale and with great effect during the eclipse of 1893. The light proceeding from the luminous ring round the dark moon was made to give us a series of rings, representing each bright line seen by the ordinary method on a photographic plate. The observers this time were stationed in West Africa and in Brazil. The African station was up one of the rivers, not very far away from the town of Bathurst. The Brazilian station was near Para Curu. The same instrument which was previously referred to as used for obtaining photographs of the stars was sent to the African station in order that photographs of the eclipse of the sun might be taken on exactly the same scale as the photographs of the stars had been, so that the stellar and solar records in the photographs might be compared. The results obtained by Messrs. Fowler and Shackleton, who were in charge of the instruments at the two stations, will be gathered from the accompanying diagrams, Figs. 10 and 11.

We get more or less complete rings when we are dealing with an extended arc of the chromosphere, or lines of dots when any small part of it is being subjected to a disturbance which increases the temperature and, possibly, the numbers of the different vapours present.

The efficiency of this method of work with the dispersion employed turns out to be simply marvellous, and in securing such valuable and permanent records as these, we have done very much better than if we had contented ourselves with the style of observations that I have referred to as having been made in 1871.

As was expected the comparison between solar and stellar records thus rendered possible enabled a very great advance to be made.

On examining these eclipse records, we find that we have to do exactly with those unknown lines which had already been photographed in the stars and in the nebulæ.

As was to be expected we, of course, deal with the lines

FIG. 10.—Untouched reproduction of photograph (African station) taken very shortly after the commencement of totality, the exposure being " instantaneous ". At this phase of the eclipse a considerable arc of the chromosphere was visible, and its spectrum is therefore shown in addition to the spectrum of the higher reaches of some of the large prominences extending beyond the moon's limb. It will be seen that at H and K there are long arcs of chromosphere and prominences, the absent portions being of course obscured by the moon. One very small prominence is especially rich in lines.

recorded in the first observations of the solar disturbances, and chronicled in that table of Professor Young's to which I have already called attention; but the important thing is the marvellously close connection between eclipse- and star-spectrum photographs so far as the "unknown lines" are concerned.

Nearly all the lines given in the table on p. 259 as visible in the Nebula of Orion and afterwards found in Bellatrix, are also among the lines photographed during the eclipse.

## DISCOVERY OF A TERRESTRIAL SOURCE OF HELIUM, 1895.

The year 1894 was made memorable by the announcement of the discovery by Lord Rayleigh and Professor Ramsay of a new gas called argon, and you know that the discovery was brought about chiefly in the first instance by the very accurate observations of Lord Rayleigh, who found that when he was determining the weight of air in the globe of a certain capacity, the weight depended upon the source from which he got the nitrogen.

From the nitrogen from atmospheric air he obtained one weight, and from that obtained by certain chemical processes he obtained another, and ultimately it was found that there was an unknown element which produced these results, these various changes in the weight, and as a consequence we had the 1895 discovery of argon.

Early in 1895 it struck Mr. Miers, of the British Museum, that it might be desirable to draw attention to the nitrogen which we have seen Dr. Hillebrand in 1888 obtaining from his crystal of uraninite; his observations, of course, were more in the mind of Mr. Miers than in the minds of the pure chemists. He therefore communicated with Professor Ramsay, who lost no time, because it was very interesting to study every possible source of nitrogen and see what its behaviour was in regard to the quantity of argon that it produced, and in the relation generally of the gas to the argon which was produced from it.

Professor Ramsay treated uraninite in exactly the same

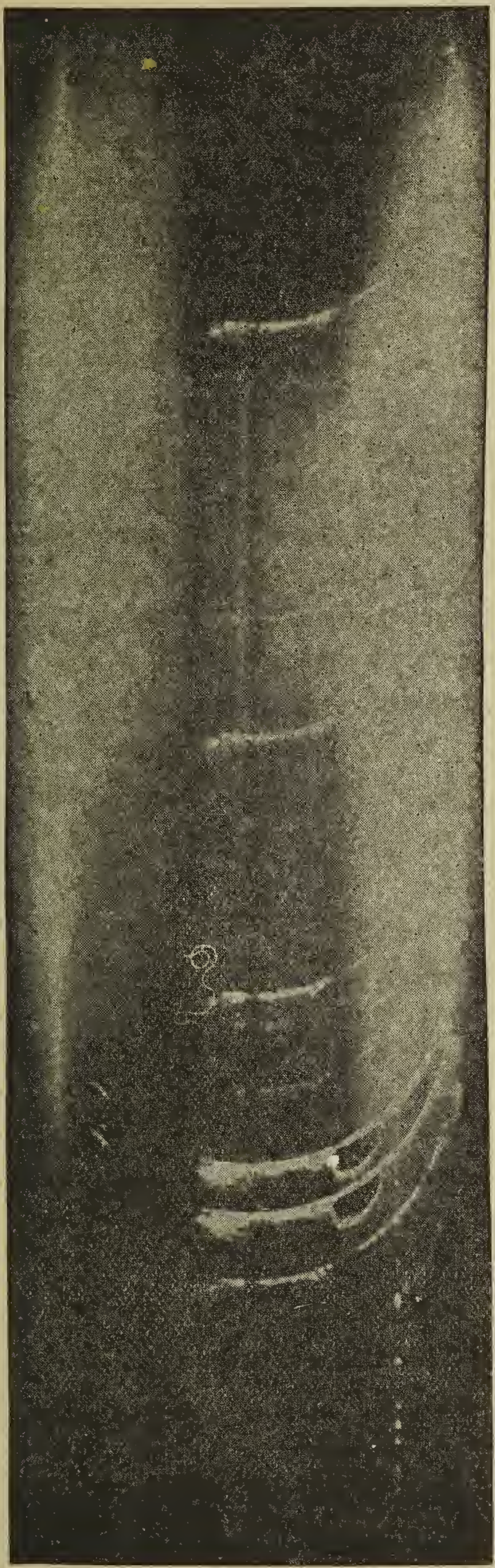


Fig. 11.—Photograph 21 (African station) taken shortly before the end of totality. A portion of the chromosphere on the other edge of the dark moon is now visible in addition to numerous prominences. It will be seen that one of the smallest prominences is rich in lines, and closely resembles that which appears in Fig. 11.

way that Dr. Hillebrand had done in 1888. The gas obtained as Dr. Hillebrand had obtained it was eventually submitted to a spectroscopic test, following Dr. Hillebrand's example. But here a noteworthy thing comes in.

*It so happened* that the pressure and electrical conditions employed by Professor Ramsay were so different from those used by Dr. Hillebrand that, although nitrogen was undoubtedly present, the fluted spectrum which, as I have previously stated, floods the orange part of the spectrum with luminous details, was absent. But still there was *something* there.

Judge of Professor Ramsay's surprise when he found that he got a bright orange line; that was the chief thing, and *not* the strong suggestion of the spectrum of nitrogen. Careful measurements indicated that the twenty-six-year-old helium had at last been run to earth, $D^3$ was at last visible in a laboratory. Professor Ramsay was good enough to send specimens of the tubes containing this gas round to other people, and he sent one of them to me.

I received Professor Ramsay's tube on 28th March, but it was not suitable for the experiments I wished to make.

On 29th March, therefore, as Professor Ramsay was absent from England, in order not to lose time I determined to see whether the gas which had been obtained by chemical processes would not come over by heating *in vacuo*, after the manner described by me to the Royal Society in 1879,[1] and Mr. L. Fletcher was kind enough to give me some particles of uraninite (bröggerite) to enable me to make the experiment.

This I did on 30th March, and it succeeded; the gas giving the yellow line came over, associated with hydrogen, in good quantity.

From 30th March onwards my assistants and myself had a very exciting time. One by one the unknown lines I had observed in the sun in 1868 were found to belong to the gas I was distilling from bröggerite; not only $D^3$ but 4923, 5017, 4471 (Lorenzoni's *f*), 6677 (the B C of Fig. 7), referred to previously, and many other solar lines, were all caught in a few weeks.

[1] *Roy. Soc. Proc.*, vol. xxix., p. 266.

But this was by no means all. The solar observations had been made by eye, and referred therefore to the less refrangible part of the spectrum, but I had obtained and studied hundreds of stellar photographs, so I at once proceeded to photograph the gas and compare its more refrangible lines with stellar lines.

Here, if possible, the result was still more marvellous. In the few-lined stars by 6th May I had caught nearly all the most important lines at the first casts of the spectroscopic net. Fig. 15, which includes some later results, will give an idea of the tremendous revelation which had been made as to the chemistry of some of the stages of star-life. I pointed out on 8th May that we had already "run home" the most important lines in the spectra of Group III. in which stars alone we find $D^3$ reversed.

These results enabled us at once to understand how it was that the "unknown lines" had been seen both in the sun's chromosphere and some nebulæ and stars. The gas obtained from the minerals made its appearance in the various heavenly bodies in which the conditions of the highest temperatures were present; and the more the work goes on, we find that this gas is really the origin of most, but certainly not of *all*, of the unknown lines which have been teasing astronomical workers for the last quarter of a century.

## THE FIRST INVESTIGATIONS OF THE SPECTRUM OF THE GAS FROM CLEVEITE.

The dates of the papers communicated to the Royal Society recording the observations of the lines in the gas obtained from minerals which had been previously recorded are as follows :—

| | | | |
|---|---|---|---|
| 25th April, - - - | 4471 | 4144 | |
| 8th May, - - - | 667 | 4388 | 4026 |
| 9th May, - - - | 3889 | | |
| 28th May, - - - | 7065 | | |
| 29th May, - - - | 5048 | 5016 | 4922 |

The lines at 667 and 5016 had been previously seen by Thalén (*Comptes Rendus*, 16th April, 1895).

Although the general distribution and intensities of the lines in the gases from bröggerite and cleveite sufficiently corresponded with some of the chief "unknown lines" in the solar chromosphere and some of the stars to render identity probable, it was necessary to see how far the conclusion was sustained by detailed investigations of the wave-lengths of the various lines.

## EMPLOYMENT OF HIGH DISPERSION.

This was practically a separate branch of the work, as the observations had to be made in the observatory. Next I give here the observations relating to $D^3$, 4471.

*The Orange Line*, $\lambda$ 5875·9.—Immediately on receiving from Professor Ramsay, on 28th March, a small bulb of the gas obtained from cleveite, a provisional determination of wave-length was made by Mr. Fowler and myself, in the absence of the sun, by micrometric comparisons with the D lines of sodium, the resulting wave-length being 5876·07 on Rowland's scale. It was at once apparent, therefore, that the gas line was not far removed from the chromospheric $D_3$, the wave-length of which is given by Rowland as 5875·98.

The bulb being too much blackened by sparking to give sufficient luminosity for further measurements, I set about preparing some of the gas for myself by heating bröggerite *in vacuo*, in the manner I have already described. A new measurement was thus secured on 30th March, with a spectroscope having a dense Jena glass prism of 60°; this gave the wave-length 5876·0.

On 5th April, I attempted to make a direct comparison with the chromospheric line, but though the lines were shown to be excessively near to each other, the observations were not regarded as final.

Professor Ramsay having been kind enough to furnish me, on 1st May, with a vacuum tube which showed the orange line very brilliantly, a further comparison with the chromosphere was made on 4th May. The observations were made by Mr. Fowler, in the third order spectrum of a grating having 14,438 lines to the inch, and the observing

telescope was fitted with a high power micrometer eye-piece; the dispersion was sufficient to easily show the difference of position of the $D_3$ line on the east and west limbs, due to the sun's rotation. Observations of the chromosphere were therefore confined to the poles.

During the short time that the tube retained its great brilliancy, a faint line, a little less refrangible than the bright orange one, and making a close double with it, was readily seen; but afterwards a sudden change took place, and the lines almost faded away. While the gas line was brilliant, it was found to be "the least trace more refrangible than $D_3$, about the thickness of the line itself, which was but narrow" ("Observatory Note Book"). The sudden diminution in the brightness of the lines made subsequent observations less certain, but the instrumental conditions being slightly varied, it was thought that the gas line was probably less refrangible than the $D_3$ line by about the same amount that the first observation showed it to be more refrangible. Giving the observations equal weight, the gas line would thus appear to be probably coincident with the middle of the chromospheric line, but if extra weight be given to the first observation, made under much more favourable conditions, the gas line would be slightly more refrangible than the middle of the chromosphere line.

Pressure of other work did not permit the continuation of the comparisons. In the meantime, Runge and Paschen announced (*Nature*, vol. lii., p. 128) that they also had seen the orange line of the cleveite gas to be a close double, neither component having exactly the same wave-length as $D_3$, according to Rowland.

They give the wave-length of the brightest component as 5878·883, and the distance apart of the lines as 0·323.

This independent confirmation of the duplicity of the gas line led me to carefully re-observe the $D_3$ line in the chromosphere for evidences of doubling. On 14th June observations were made by Mr. Shackleton and myself of the $D_3$ line in the third and fourth order spectra under favourable conditions; "the line was seen best in the fourth order, on an extension of the chromosphere or prominence

on the north-east limb of the sun. The $D_3$ line was seen very well, having every appearance of being double, with a faint component on the red side, dimming away gradually; the line of demarcation between the components was not well marked, but it was seen better in the prominence than anywhere else on the limb" ("Observatory Note Book").

It became clear, then, that the middle of the chromosphere line, as ordinarily seen, and as taken in the comparison of 4th May, does not represent the place of the brightest component of the double line, so that exact coincidence was not to be expected.

The circumstance that the line is double in both gas and chromosphere spectrum, in each the less refrangible component being the fainter, taken in conjunction with the direct comparisons which have been made, rendered it highly probable that one of the gases obtained from cleveite is identical with that which produces the $D_3$ line in the spectrum of the chromosphere.

Other observers have since succeeded in resolving the chromospheric line. On 20th June, Professor Hale found the line to be clearly double in the spectrum of a prominence, the less refrangible component being the fainter, and the distance apart of the lines being measured as 0·357 tenth-metres (*Ast. Nach.*, 3302).

The doubling was noted with much less distinctness in the spectrum of the chromosphere itself on 24th June. Professor Hale points out that Rowland's value of the wave-length (as well as that of 5875·924, determined by himself on 19th and 20th June) does not take account of the fact that the line is a close double.

Dr. Huggins, after some failures, observed the $D_3$ line to be double on 10th July (*Ast. Nach.*, 3302); he also notes that the less refrangible component was the fainter, and that the distance apart of the lines was about the same as that of the lines in the gas from cleveite, according to Runge and Paschen.

It may be added, that in addition to appearing in the chromosphere, the $D_3$ line has been observed as a bright line in nebulæ by Dr. Copeland, Professor Keeler and

others; in $\beta$ Lyræ and other bright line stars; and as a dark line in such stars as Bellatrix, by Mr. Fowler, Professor Campbell and Professor Keeler. In all these cases it is associated with other lines, which, as I shall show presently, are associated with it in the spectra of the new gases.

*The Blue Line*, $\lambda$ 4471·8.—A provisional determination on 2nd April of the wave-length of a bright blue line, seen in the spectrum of the gases obtained from a specimen of cleveite, showed that it approximated very closely to a chromospheric line, the frequency of which is stated as 100 by Young.

This line was also seen very brilliantly in the tube supplied to me by Professor Ramsay on 1st May, and on 6th May it was compared directly with the chromosphere line by Mr. Fowler. The second order grating spectrum was employed. The observations in this region were not so easy as in the case of $D_3$, but with the dispersion employed, the gas line was found to be coincident with the chromospheric one. In this case also, the chromosphere was observed at the sun's poles, in order to eliminate the effects due to the sun's rotation.

Besides appearing in the spectrum of the chromosphere, the line in question is one of the first importance in the spectra of nebulæ, bright line stars, and of the white stars such as Bellatrix and Rigel.

*The Infra-red Line*, $\lambda$ 7065·5.—In addition to $D_3$ and the line at 4471·8, there is a chromospheric line in the infra-red which also has a frequency of 100, according to Young. On 28th May I communicated a note to the Royal Society stating that this line had been observed in the spectrum of the gases obtained from bröggerite and euxenite (*Roy. Soc. Proc.*, vol. lviii., p. 192), solar comparisons having convinced me that the wave-length of the gas line corresponded with that given by Young; and I added: "It follows, therefore, that besides the hydrogen lines all three chromospheric lines in Young's list which have a frequency of 100 have now been recorded in the spectra of the new gas or gases obtained from minerals by the distillation method".

M. Deslandres, of the Paris Observatory, has also

observed the line at 7065 in the gas obtained from the cleveite (*Comptes Rendus*, 17th June, 1895, p. 1331).

A great deal of work has been done upon these gases from other points of view than those which affect their cosmical relations, and perhaps I may be allowed next to refer to some of the results which have been obtained by myself.

### HELIUM NOT CONNECTED WITH ARGON.

The first point is that the gas from the minerals contains no argon. Dr. Ramsay in his first experiments came to the conclusion that the spectra of argon and helium contained many common lines; indeed at first the observed coincidences were so remarkable that he came to the conclusion that the connection was so close that atmospheric argon contained a gas absent from the argon seen in his helium tube.

This statement was subsequently withdrawn, but the compound nature both of argon and helium was suggested by the fact that there were lines common to the two gases. These lines were in the red; one coincidence I found broke down with moderate dispersion, the other yielded subsequently to the still greater dispersion employed by Drs. Runge and Paschen. It may be also stated here that I have not found a single coincidence between argon and any line in the spectrum of any celestial body whatever. This happens, as everybody knows, also in the case of oxygen, nitrogen, chlorine, and the like.

### THE CLEVEITE GAS A COMPOUND.

The first spectroscopic observations made it perfectly obvious that the gas as obtained from uraninite is a mixture of gases, that the gas which gives the yellow line is not an isolated one, but is mixed up with other gases which give other lines.

In May I wrote as follows:—[1]

"The preliminary reconnaissance suggests that the gas obtained from bröggerite by my method is one of complex origin.

[1] *Proc. R. S.*, lviii., p. 114.

"I now proceed to show that the same conclusion holds good for the gases obtained by Professors Ramsay and Clève from cleveite.

"For this purpose, as the final measures of the lines of the gas as obtained from cleveite by Professors Ramsay and Clève have not yet been published, I take those given by Crookes and Clève, as observed by Thalén.

"The most definite and striking result so far obtained is that in the spectra of the minerals giving the yellow line I have so far examined, I have never once seen the lines recorded by Crookes and Thalén in the blue. This demonstrates that the gas obtained from certain specimens of cleveite by chemical methods is vastly different from that obtained by my method from certain specimens of bröggerite, and since, from the point of view of the blue lines, the spectrum of the gas obtained from cleveite is more complex than that of bröggerite, the gas itself cannot be more simple.

"Even the blue lines themselves, instead of appearing *en bloc*, vary enormously in the sun, the appearances being

4922 (4921·3) = thirty times
4713 (4712·5) = twice.

"These are not the only facts which can be adduced to suggest that the gas from cleveite is as complex as that from bröggerite."

It is seen that quite early in the inquiry we had not only spectroscopic evidence in the laboratory which was complete in itself, but that the case was greatly strengthened when the behaviour of the various lines in the sun and stars was also brought into evidence.

In the first case we had the laboratory separation of $D_3$ from the lines 5048, 5016, and 4922.

Later on in the same month I showed that the lines at $D_3$ and 447 behaved in one way, and that at 667 behaved in another.

In order to test this view I made some observations based on the following considerations:—

(1) In a simple gas like hydrogen, when the tension of the electric current given by an induction coil is increased

by inserting first a jar and then an air-break into the circuit, the effect is to increase the brilliancy and the breadth of all the lines, the brilliancy and breadth being greatest when the longest air-break is used.

(2) Contrariwise, when we are dealing with a known compound gas; at the lowest tension we may get the complete spectrum of the compound without any trace of its constituents, and we may then, by increasing the tension, gradually bring in the lines of the constituents, until, when complete dissociation is finally reached, the spectrum of the compound itself disappears.

Working on these lines the spectrum of the spark at atmospheric pressure passing through the gas or gases, distilled from bröggerite, has been studied with reference to the special lines C (hydrogen), $D_3$, 667, and 447.

The first result is that all the lines do not vary equally as they should do if we were dealing with a simple gas.

The second result is that at the lowest tension 667 is relatively more brilliant than the other lines; on increasing the tension C and $D_3$ considerably increase their brilliancy, 667 relatively and absolutely becoming more feeble, while 447, seen easily as a narrow line at low tension, is almost broadened out into invisibility as the tension is increased in some of the tubes, or is greatly brightened as well as broadened in others (Fig. 12).

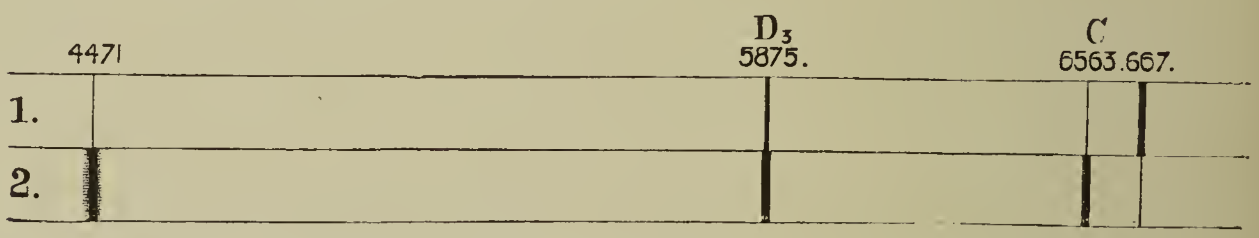



FIG. 12.—Diagram showing changes in intensities of lines brought about by varying the tension of the spark. 1. Without air-break. 2. With air-break.

The above observations were made with a battery of five Grove cells; the reduction of cells from 5 to 2 made no difference in the phenomena except in reducing their brilliancy.

Reasoning from the above observations it seems evident that the effect of the higher tension is to break up a compound or compounds, of which C, $D_3$, and 447 represent constituent elements; while, at the same time, it would

appear that 667 represents a line of some compound which is simultaneously dissociated.

The unequal behaviour of the lines has been further noted in another experiment, in which the products of distillation of bröggerite were observed in a vacuum tube and photographed at various stages. After the first heating $D_3$ and 4471 were seen bright, before any lines other than those of carbon and hydrogen made their appearance. With continued heating 667, 5016, and 492 also appeared, although there was no notable increase of brightness in the yellow line; still further heating introduced additional lines, 5048 and 6347.

These changes are represented graphically in the following diagram (Fig. 13).

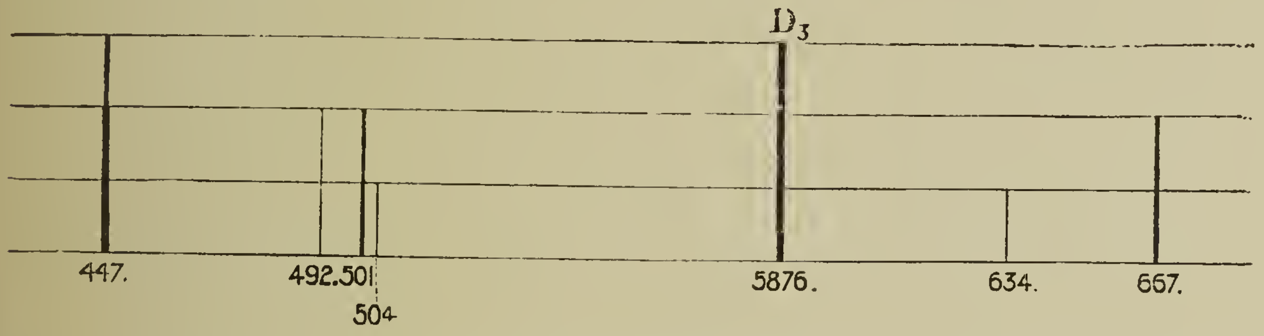



Fig. 13.—Diagram showing order in which lines appear in spectrum of vacuum tube when bröggerite is heated.

It was recorded further that the yellow line was at times dimmed, while the other lines were brightened.

In my second note, communicated to the Royal Society on the 8th May, I stated that I had never once seen the lines recorded by Thalén in the blue, at λ 4922 and 4715.

It now seems possible that their absence from my previous tubes was due to the fact that the heating of the minerals was not sufficiently prolonged to bring out the gases producing these lines.

It is perhaps to the similar high complexity of the gas obtained from cleveite that the curious behaviour of a tube which Professor Ramsay was so good as to send me, must be ascribed. When I received it from him the glorious yellow effulgence of the capillary while the current was passing was a sight to see. But after this had gone on for some time, while the coincidence of the yellow line with $D_3$ of the chromosphere was being inquired into, the luminosity of the tube was considerably reduced, and the colours

in the capillary and near the poles were changed. From the capillary there was but a feeble glimmer, not of an orange tint, while the orange tint was now observed near the poles, the poles themselves being obscured by a coating on the glass of brilliant metallic lustre.

After attempting in vain for some time to determine the cause of the inversion of $D_3$ and 447 in various photographs I had obtained of the spectra of the products of distillation of many minerals, it struck me that these results might be associated with the phenomena exhibited by the tube, and that one explanation would be rendered more probable if it could be shown that the change in the illumination of the tube was due to the formation of platinum compounds, platinum poles being used. On 21st May I accordingly passed the current and heated one of the poles, rapidly changing its direction to assure the action of the negative pole, when the capillary shortly gave a very strong spectrum of hydrogen, both lines and structure. A gentle heat was continued for some time, and apparently the pressure in the tube varied very considerably, for as it cooled the hydrogen disappeared and the $D_3$ line shone out with its pristine brilliancy. The experiment was repeated on 24th May, and similar phenomena were observed.

Some little time after[1] Professors Runge and Paschen, from an entirely different standpoint, arrived at exactly the same conclusion.

The employment of exposures extending over seven hours has given a considerable extension in the number of lines, and the bolometer has been called in to investigate lines in the infra-red; better still, they have employed well-practised hands in searching for series of lines. Operating by chemical means upon a crystal of cleveite free from any other mineral, they have obtained a product so pure that from these series there are no outstanding lines. Very great weight, therefore, must be attached to their conclusions.

As a result of their investigations Drs. Runge and Paschen stated that the gas given off even by a pure crystal

[1] *Nature*, 26th September, 1895.

of cleveite is not simple. In their view the mixture consists of two constituents.

This conclusion was arrived at from the following considerations. "The wave-lengths λ of the lines belonging to the same series are always approximately connected by a formula somewhat similar to Balmer's—

$$1/\lambda = A - B/m^2 - C/m^4.$$

A determines the end of the series towards which the lines approach for high values of *m*, but does not influence the difference of wave-numbers of any two lines. B has nearly the same value for all the series observed, and C may be said to determine the spread of the series, corresponding intervals between the wave-numbers being larger for larger values of C. As B is approximately known two wave-lengths of a series suffice to determine the constants A and C, and thus to calculate approximately the wave-lengths of the other lines. It was by this means that we succeeded in disentangling the spectrum of the gas in cleveite, and showing its regularity.

"In the spectrum of many elements two series have been observed for which A has the same value, so that they both approach to the same limit. In all these cases the series for which C has the smaller value, that is to say, which has the smaller spread, is the stronger of the two. In the spectrum of the gas in cleveite we have two instances of the same occurrence. One of the two pairs of series, the one to which the strong yellow double line belongs, consists throughout of double lines whose wave-numbers seem to have the same difference, while the lines of the other pair of series appear to be all single. Lithium is an instance of a pair of series of single lines approaching to the same limit. But there are also many instances of two series of double lines of equal difference of wave-numbers ending at the same place as sodium, potassium, aluminium, etc. There are also cases where the members of each series consist of triplets of the same difference of wave-numbers, as in the spectrum of magnesium, calcium, strontium, zinc, cadmium, mercury. But there is no instance of an element whose spectrum contains two pairs of series ending at the same place. This

suggested to us the idea that the two pairs of series belonged to different elements. One of the two pairs being by far the stronger, we assume that the stronger one of the two remaining series belongs to the same element as the stronger pair. We thus get two spectra consisting of three series each, two series ending at the same place, and the third leaping over the first two in large bounds and ending in the more refrangible part of the spectrum. This third series we suppose to be analogous to the so-called principal series in the spectra of the alkalis, which show the same features. It is not impossible, one may even say not unlikely, that there are principal series in the spectra of the other elements. But so far they have not been shown to exist.

"Each of our two spectra now shows a close analogy to the spectra of the alkalis.

"We therefore believe the gas in cleveite to consist of two, and not more than two, constituents."

To the one containing the line $D_3$, which I discovered in 1868, the name helium remains; the other for the present we may call "gas X".[1]

The chief lines of these two constituents are as follows, according to Runge and Paschen, the wave-lengths being abridged to five figures.

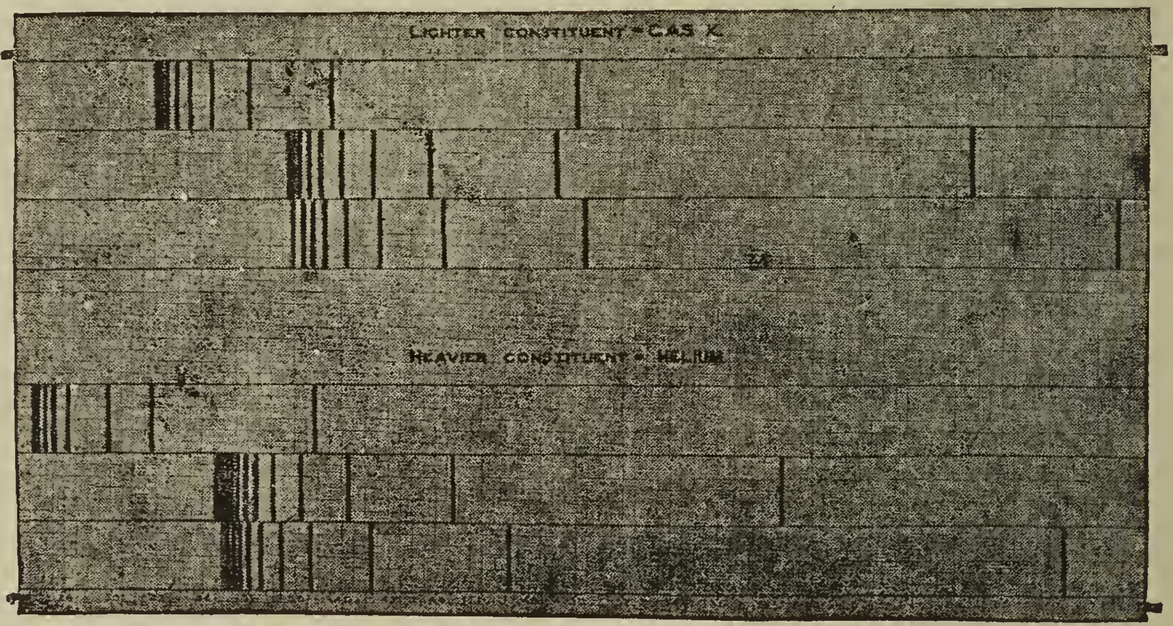



FIG. 14.—Runge and Paschen's results suggesting that cleveite gives off two gases, each with three series of lines.

[1] In the many comparisons I had to make, I soon found the inconvenience of not having a name for the gas which gave 667, 501 and other lines. When, therefore, Professors Runge and Paschen, who had endorsed

## HELIUM.

| Principal Series. | 1st Subordinate Series. | 2nd Subordinate Series. |
|---|---|---|
| 2663·3 | 3456·9 ? | 3481·6 |
| 2677·2 | 3461·4 ? | 3490·8 |
| 2696·2 | 3466·0 | 3502·5 |
| 2723·3 | 3471·9 | 3517·5 |
| 2763·9 | 3479·1 | 3537·0 |
| 2829·2 | 3487·9 | 3563·1 |
| 2945·2 | 3498·8 | 3733·0 |
| 3187·8 | 3512·6 | 3867·6 |
| 3888·8 | 3530·6 | 4121·0 |
| | 3554·6 | 4713·3 |
| | 3587·4 | 7065·5 |
| | 3634·4 | |
| | 3705·1 | |
| | 3819·8 | |
| | 4026·3 | |
| | 4471·6 | |
| | 5875·8 | |

## GAS X.

| Principal Series. | 1st Subordinate Series. | 2nd Subordinate Series. |
|---|---|---|
| 3176·6 | 3756·2 | 3770·7 |
| 3196·8 | 3768·9 | 3787·6 |
| 3211·6 | 3785·0 | 3838·2 |
| 3231·3 | 3805·9 | 3878·3 |
| 3258·3 | 3833·7 | 3936·0 |
| 3296·9 | 3872·0 | 4024·1 |
| 3354·7 | 3926·7 | 4169·1 |
| 3447·7 | 4009·4 | 4437·7 |
| 3613·8 | 4143·9 | 5047·8 |
| 3964·9 | 4388·1 | 7281·8 |
| 5015·7 | 4922·1 | |
| | 6678·4 | |

my results, and had extended them, called upon me, I thought it right to suggest to them that, sinking the priority of my own results, we should all three combine in suggesting a name. Professor Runge (under date 20th October) wrote me: "The inference that there are two gases is a spectroscopical one, being based on the investigation of the 'series'. Now, though we think this basis quite sound, we must own that the conclusion rests on induction. . . . For this reason we do not want to give a name to 'gas X'." I have so far suggested no name, though Orionium and Asterium have been in my mind.

More recently Professor Ramsay has abandoned his view of the simple nature of the cleveite gas, and states that from his experiments "there appears ground for the supposition that helium is a mixture".[1]

THE EXISTENCE OF THE NEW GASES IN CELESTIAL BODIES.

And now comes the great revelation, and it is this. The majority of the lines classed as unknown in the spectra of the Orion nebula, stars of Group III. and the sun are really due to the cleveite gases.

The following table sets this result out. It will be seen that of seventeen unknown lines, twelve have been run to earth.

*COMPARISON OF UNKNOWN (PREVIOUS TO HE. AND X) LINES IN ORION NEBULA AND BELLATRIX.*

| Orion Nebula. | | Bellatrix and Eclipse, 1893. | Origin. |
|---|---|---|---|
| Campbell. | Lockyer. | | |
| 3869 | *3869 (7) | †3867·5 (Falls | He. |
| 3889 | 3888 (7) | 3888 on He.) | He. |
| — | 4011 (3) | 4009 (8) | X |
| 4026 | 4026 (5) | 4026 (10) | He. |
| — | 4042 (1) | 4041 (3) | Still Unknown |
| 4067 | 4068 (3) | 4070 (3) | Still Unknown |
| 4121 | 4121 (1) | 4121·3 (7) | He. |
| 4143 | 4143 (1) | 4144 (8) | X |
| — | 4168 (1) | 4169 (5) | X |
| 4265 | 4270 (3) | 4268 (7) | Still Unknown |
| 4389 | 4390 (3) | 4389 (8) | X |
| 4472 | 4472 (7) | 4472 (10) | He. |
| — | 4540 (3) | 4541 (1) | Still Unknown |
| — | 4628 (3) | 4630 (3) | Still Unknown |
| 4714 | 4716 (3) | 4715 (5) | He. |
| — | *4924 (5) | †4922·1 (8) | X |
| 5874 | 5875·8 | 5875·8 | $D_3$ He. |

* Between these λλ there are forty-two lines in the Orion photograph of which six are known other than He. and X.

† Between these λλ there are forty-five lines in the Bellatrix photograph of which five are known other than He. and X.

The following tables give the complete list of lines and the celestial body in which they have been traced.

[1] *Nature*, vol. liii., p. 598.

In the tables, under "sun," C, followed by a number, indicates the frequency as given by Young; E indicates the lines photographed during the eclipse of 1893. Under "star or nebula" the references are to the tables given in my memoir on the nebula of Orion (*Phil. Trans.*, vol. clxxxvi., 1895, p. 86 *et seq.* N = Nebula of Orion).

HELIUM.

| 11220. | Sun. | Star or Nebula. |
|---|---|---|
| 3889 | C E | N. III. $\gamma$ |
| 3188 | | |
| 2945 | | |
| 2829 | | |
| 2764 * | | |
| 2723 | | |
| 2696 | | |
| 2677 | | |
| | | |
| 5876 | C 100 E | |
| 4472 | C 100 E | |
| 4026 | C 25 E | |
| 3820 | E | $\alpha$ Cygni |
| 3705 | | |
| 3634 | | |
| 3587 | | |
| 3555 | | |
| 3513 | | |
| 3499 * | | |
| 3488 | | |
| 3479 | | |
| 3472 | | |
| 3466 | | |
| 3461 | | |
| | | |
| 7066 | C 100 | |
| 4713 | C 2 E | |
| 4121 | E | N. $\alpha$ Cygni |
| 3868 | ? | |
| 3777 | E | Bellatrix |
| 3652 | | |
| 3599 | | |
| 3567 | | |
| 3537 | | |
| 3517 * | | |
| 3503 | | |
| 3491 | | |
| 3482 | | |

* Means that these lines are out of the range of my observations.

GAS X.

| | Sun. | Star or Nebula. |
|---|---|---|
| 5016 | C 30 E | |
| 3965 | ? | III. $\gamma$ |
| 3614* | E | |
| 3448* | | |
| 3355* | | |
| 3297* | | |
| 3258* | | |
| 3231* | | |
| 3213* | | |
| 6678 | C 25 | |
| 4922 | C 30 E | |
| 4388 | E | N. III. $\gamma$ |
| 4144 | E | III. $\gamma$ |
| 4009 | | III. $\gamma$ |
| 3927 | | Bellatrix |
| 3872 | | Bellatrix |
| 3833 | E | Hid by H. line |
| 3806 | | Bellatrix |
| 3785* | | |
| 7282 | | |
| 5048 | C 2 | |
| 4438 | | Bellatrix |
| 4169 | | Bellatrix |
| 4024 | ? | N. III. $\gamma$ |
| 3936 | Hid in K. | |
| 3878 | C E | $\alpha$ Cygni |
| 3838 | C E | $\alpha$ Cygni |
| 3803* | | |

* Means that these lines are out of the range of my observations.

The annexed reproduction of a photograph of Bellatrix will show how striking has been the result of the discovery so far as stellar spectra are concerned.

Hydrogen, helium and gas X are thus proved to be those elements which are, we may say, completely represented in the hottest stars and in the hottest part of the sun that we can get at. Here then, in 1895, we have abundant confirmation of the views I put forward in 1868 as to the close connection between helium and hydrogen.

## EFFECTS OF DIFFUSION.

A diffusion experiment described in their paper enabled Messrs. Runge and Paschen to go a stage farther, and to

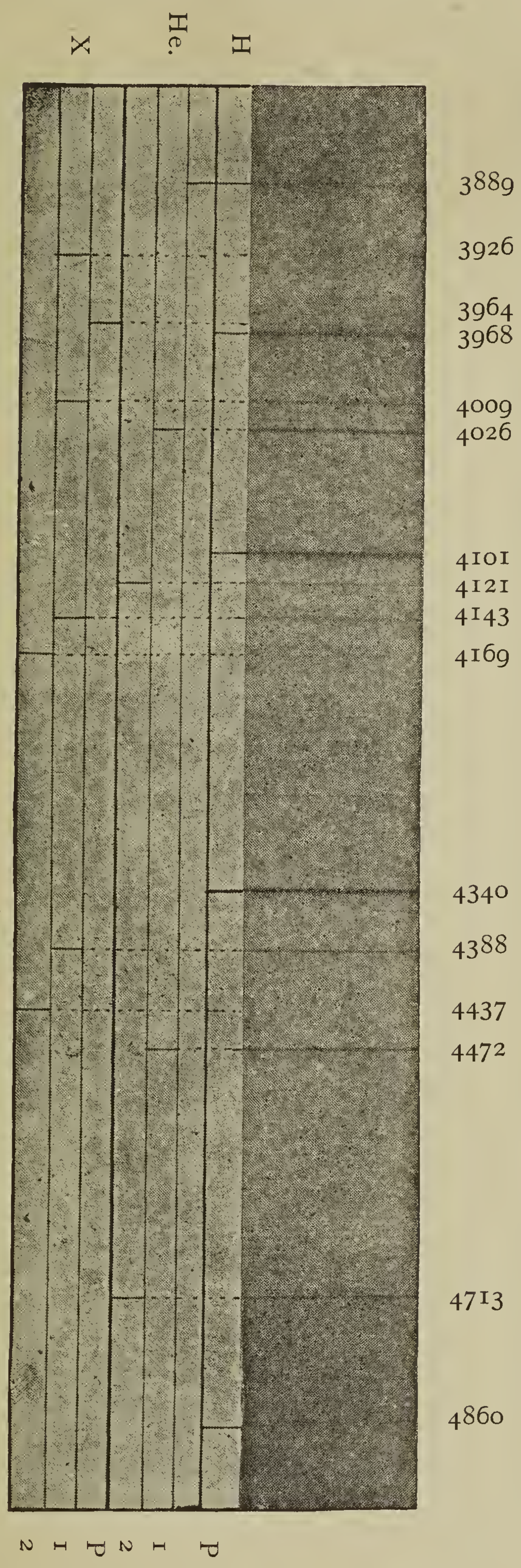



FIG. 15.—The spectrum of Bellatrix showing the lines of hydrogen and those which have been traced to the gases obtained from minerals.

announce that of their two constituents the gas-giving $D^3$ was the heavier one. They also add :—

"From the fact that the second set of series is on the

whole situated more to the refrangible part of the spectrum, one may, independently of the diffusion experiment, conclude that the element corresponding to the second set is the heavier of the two".

As they themselves pointed out, however, the result was not final, because the pressures were not the same. I have recently made some experiments in which the pressures remain the same.

An U tube was taken, and at the bend was fixed a plaster of Paris plug about 1·5 c.m. thick; in one of the limbs two platinum wires were inserted. The plug was saturated with hydrogen to free it from air; the tube was then plunged into a mercury trough, and fixed upright with the limbs full of mercury. Into the leg (A) with the platinum wires a small quantity of hydrogen was passed, and as soon after as possible another small quantity of a mixture of helium and hydrogen from samarskite was put up the other limb (B) of the U tube.

Immediately after the helium was passed into the limb (B) spectroscopic observations were made of the gas in the limb (A); $D_3$ was already visible, and there was no trace of 5015·7. This result seems to clearly indicate that if a true diffusion of one constituent takes place, the component which gives $D_3$ is lighter than the one which gives the lines at wave-length 5015·7.

Although this result is opposed to the statement made by Runge and Paschen, it is entirely in harmony with the solar and stellar results.

In support of this I may instance that of the cleveite lines associated with hydrogen in the chromosphere and the stars of Group III. $\gamma$; those allied to $D_3$ are much stronger than those belonging to the series of which 5015·7 forms part.

## MINERALS EXAMINED.

So far I have worked upon some seventy minerals, and I have found the orange line in sixteen.

The following are the minerals, etc., which have been

investigated; those which give the $D_3$ line being marked with an asterisk:—

- *Æschynite.
- Almandine.
- Anglesite.
- Anhydrite.
- Augite.
- Barytes.
- *Bröggerite.
- Bronzite.
- Calco-uranite.
- Cassiterite.
- Celestine.
- Chalk.
- Charnockite.
- Chromite.
- *Cleveite.
- Columbite.
- Crocidolite.
- Cupro-uranite.
- *Eliasite.
- Enstatite.
- *Euxenite.
- *Fergusonite.
- Franklinite.
- Gadolinite.
- Gahnite.
- Geikielite.
- Gneiss.
- Granite.
- Graphite.
- *Gummite.
- Hæmatite.
- *Hielmite.
- Hornblende.
- Hypersthene.
- Ilmenite.
- Iridosmine.
- Kielhanite.
- Kyanite.
- Ludwigite.
- Magnesium.
- Magnetite.
- Manganese Nodule.
- Minium.
- *Monazite.
- Obsidian.
- Olivine.
- Olivine-Enstatite.
- *Orangeite.
- Orthite.
- Pitchblende.
- Plumbic Ochre.
- *Polycrase.
- *Pyrochlore.
- Quartz.
- Red Clay.
- Rhodonite.
- *Samarskite.
- Schorlomite.
- Sphene.
- Staurolite.
- Thorite.
- *Thoro-gummite.
- *Uraninite.
- Uranocircite.
- Uranophase.
- Wulfenite.
- Wolfram.
- Xenotine.
- *Yttro-Gummite.

J. NORMAN LOCKYER.

# INSULAR FLORAS.

## PART VI. (A).

IN the preceding articles I have briefly reviewed the literature relating to Insular Floras which has appeared during the last decade, and I have extracted therefrom the principal or most interesting facts, which I have given with some comments of my own. That I have been able to do this with some profit is largely due to the advantages I have enjoyed through the kindness of the Director of the Royal Gardens, Kew. Since the publication in 1885 of my first essay on this subject, in the *Botany of the Voyage of H.M.S.* "*Challenger*," all or nearly all collections of insular plants received at Kew have passed through my hands for determination and reporting on; and I have also been favoured with many notes and criticisms by travellers and other persons interested in plant distribution. I propose therefore to enter into a short recapitulation and discussion of the main facts thus accumulated; but before doing so I will refer to some more or less important contributions to the subject that have come to light during the progress of the present series of articles.[1]

It will be convenient to take the islands in the same geographical order previously followed (1), beginning with Polynesia.

There are some interesting recent contributions to the flora of Polynesia, taking the designation in its widest sense; but no one has yet attempted to bring together what is known, or ascertainable from materials preserved in herbaria, of the vegetation of the numerous small coral islands and groups of islands, more or less recently annexed by, or taken under the protection of, Great Britain. This the writer is engaged upon, and some particulars thus acquired may be

[1] A review of the additional literature having extended beyond what was expected, the discussion referred to will form the subject of a concluding article.

utilised here in dealing with the literature of the subject. Some years ago Mr. J. T. Arundel delivered a lecture at San Francisco, before the Geographical Society of the Pacific, on the Phœnix Group and other islands of the Pacific, and he has since published it (2) with additional notes. Mr. Arundel writes from actual experience, having visited a large number of the most remote islets of the Pacific and collected samples of their scanty floras, which were determined for him at Kew, where the specimens are preserved. Unfortunately several of the names of the plants in his list have undergone such a transformation as to be almost unrecognisable.

Besides the Phœnix Group, which was under his personal control, Mr. Arundel visited such out-of-the-way islands as Starbuck, Caroline (not the Caroline Group), Fanning, Malden, Palmerston and Ducie. Mr. Arundel describes Starbuck and Caroline Islands as examples of two kinds of very small islands common in the Pacific, though not confined to it. The former represents those consisting of an unbroken mass which is treeless, and indeed almost devoid of vegetation; and the latter is a typical coral atoll, consisting of a ring of islets encircling a central lagoon, and supporting a relatively luxuriant vegetation. Starbuck is very scantily furnished with vegetation, only about half a dozen species being represented. The principal plants are *Lepidium piscidium* and *Sida fallax*; both of wide range in Polynesia. Caroline Island claims a little more attention, because its history, position, conformation, meteorology, botany and zoology have been very fully worked out and illustrated. In 1883 this island was selected by the Americans, by the British, and by the French as the most suitable spot for observing the total eclipse of the sun. The American party was relatively numerous, and they drew up a somewhat elaborate report (3), illustrated chiefly by prints from photographs taken by the two gentlemen constituting the English party. These illustrations give an excellent idea of the form and vegetation of an atoll, including a bird's eye view, which enables us, better than any description could, to realise its smallness and isolation. Caroline Island

is situated in almost exactly 150° W. longitude and 10° S. latitude, and is distant, according to Mr. Arundel, about 400 miles from Tahiti, the nearest island of considerable size—say a third larger than the Isle of Wight; and 420 from Starbuck. Although in most parts well clothed with vegetation, this vegetation consists of very few, perhaps not more than twenty, species of vascular plants. Several others now exist, either as the remains of cultivation or accidental introduction; and the abundance of the cocoanut palm is due to planting, which has now been in operation for some years. Whether the cocoanut existed in the island on the first advent of man there is no evidence to show; but there are trees of other kinds of large size, as depicted and described in the report referred to. They are: *Calophyllum Inophyllum* (Guttiferæ), *Morinda citrifolia* (Rubiaceæ), *Cordia subcordata* (Boragineæ), *Pisonia grandis* (Nyetaginaceæ), and a screw pine, probably the widely spread *Pandanus odoratissimus*. One of the illustrations is a most effective representation of a group of screw pines. The *Cordia* is perhaps the commonest tree, and is most conspicuous, having a spreading crown with branches down to the ground. *Pisonia grandis* is described as forty or fifty feet high, with a trunk four feet in diameter; dimensions one would hardly have expected. I have drawn somewhat freely from this report, because it is by far the most instructive known to me.

A more recent contribution to island literature by Mr. C. M. Woodford (4) is equally deserving of attention, though wanting illustrations. It deals with the Gilbert Archipelago, one of the most remarkable of the numerous groups in the Eastern Pacific. There are sixteen islands, not counting the islets of the atolls, forming a chain, trending from north-west to south-east and extending from about 3° north to 3° south latitude in 173° to 177° east longitude. Eleven out of the sixteen are of atoll formation, and the largest of them is little more than twenty miles long and twenty feet high in the highest part. They are mostly inhabited, and the population half a century ago was estimated at 50,000, though it has since dwindled down to

probably a quarter of that number. The presence of so large a population must have had some modifying influence on the vegetation; yet not to the extent that might have been expected, because there is little cultivation, the natives living largely on fish, with which the waters swarm. Mr. Woodford says: "The islands are clothed from end to end with a dense growth of cocoanut palms and other vegetation, and present a beautiful appearance when approaching from the sea. The reefs and lagoons teem with fish, thus enabling the islands to support a population which for their land area was at one time equalled in no part of the world."

Mr. Woodford gives a list of the plants compiled from observations on the islands he visited, which he believes is nearly complete. As I am able to supplement it by a few additional species in the Kew Herbarium, chiefly collected by the Rev. Mr. Whitmee, and also to supply specific names in some cases where he gives only the generic, I will give a list of all the vascular plants known to inhabit the group, as a sample of the typical coral island flora. *Calophyllum Inophyllum* (Gutti feræ), *Sida fallax* (Malvaceæ), *Triumfetta procumbens* (Tiliaceæ), *Tribulus cistoides* (Zygophyllaceæ), *Pemphis acidula* (Lytheraceæ), *Rhizophora mucronata* (Rhizophoraceæ), *Guettarda speciosa* and *Morinda citrifolia* (Rubiaceæ), *Scævola Kœnigii* (Goodeniaceæ), *Tournefortia argentea* (Boraginaceæ), *Pisonia inermis* and *Boerhaavia diffusa* (Nyctaginaceæ), *Euphorbia Atoto?* (Euphorbiaceæ), *Ficus tinctoria* (Moraceæ), *Crinum pedunculatum ?* (Amaryllidaceæ), *Cocos nucifera* (Palmaceæ), *Pandanus odoratissimus* (Pandanaceæ), *Fimbristylis glomerata* (Cyperaceæ), *Lepturus repens* (Gramineæ), and *Polypodium Phymatodes* (Filices)—just a score of species, it will be seen, belonging to as many different genera, and to eighteen different natural orders of the most diverse habit and structure. They are almost without exception plants of general distribution in tropical oceanic islands and on the sea-shores of the continents. The majority of them indeed inhabit the smaller remote islands of the tropical parts of the Indian Ocean. I will only add here that their seeds are such as

are transported by oceanic currents, birds, and winds, without destroying their vitality. In another article I propose discussing these agents of dispersal in some detail. The absence from the above list of the two largest natural orders—Leguminosæ and Compositæ—may cause some surprise, especially as the seeds of many of the former bear long immersion in salt water with impunity, and the pappose achenes of the latter are often, it is assumed, conveyed long distances by wind. Leguminosæ are rare in all oceanic islands, both coral and volcanic; but Compositæ, on the other hand, are characteristic of many volcanic islands, the Galapagos and St. Helena, for example.

The distribution of the plants of the Tonga or Friendly Islands has been worked out by the writer (5), and a few of the most interesting facts may be repeated here. This group lies to the south-east of Fiji, between 18° and 23° south latitude, and 173° and 176° west longitude, and comprises both volcanic and coral islands; some of the former being considerably larger than those of the Gilbert Group, and rise to altitudes of 500 to 3000 feet. Fuller information on the geology of the islands will be found in an article (6) by Mr. J. J. Lister. But although the Tonga Islands are considerably larger than the Gilbert Islands, it is more in land area and altitude than external dimensions, and it is due partly to the absence of central lagoons. Tongatabu in the south, the largest of the group, is about twenty-two miles in its greatest length, and is composed entirely of coral limestone. This island is the best known botanically; but Mr. J. J. Lister, whose collections were worked out for my paper referred to above, thoroughly explored the neighbouring smaller, though more elevated, Eua, which gave a considerable number of additional species. Since the publication of my paper, Kew has acquired a collection of dried plants made by Mr. C. S. Crosby in the Vavau cluster in the north. This collection has not yet been thoroughly worked out, but although it doubtless contains some additions, they will not be of a character to modify what has been written respecting the affinities of the flora of the whole group. The total num-

ber of assumed indigenous species of vascular plants in my enumeration is 290, whereof 246 have a westward, and 220 have an eastward extension in Polynesia; 138 are Australasian (Australia, New Zealand and outlying islands), 162 are Malayan, and at least 150 have a wider range either in the Old or New World, or in both. From the foregoing figures it will be seen that the flora of the Tonga Islands is largely composed, like the very small one of the Gilbert Islands, of species of wide distribution. Indeed no genus is peculiar to the group, and only ten species so far as our present knowledge goes are endemic, and a more complete exploration of the Fiji Islands and other neighbouring groups may reduce this number. The 290 species of the Tongan flora represent no fewer than 202 genera and seventy-nine natural orders out of the 202 recognised in Bentham and Hooker's *Genera Plantarum*. The proportions are 2·55 genera to an order, and 1·43 species to a genus in the Tongan flora. In the flora of the world the proportions I obtained by a very rough calculation are 37·50 genera to an order, and 12·65 species to a genus. Taking the number of Tongan species (138) which extend to Australasia, one might overestimate the affinities, because, as a matter of fact, a large proportion of these species have a wide range. Indeed only a dozen species have decidedly Australasian connections. These are: *Melicytus ramiflorus*, *Ratonia stipitata*, *Metrosideros polymorpha*, *Jasminum simplicifolium*, *Hoya australis*, *Ipomœa congesta*, *Pisonia inermis*, *Peperomia leptostachya*, *Euphorbia Sparmannii*, *Ficus aspera*, *Podocarpus elata* and *Pteris comans*. It will be perceived that the connections are specific rather than generic. But the most significant facts brought out in the paper under consideration are two, namely, the large proportion of species—upwards of a third—peculiar to Polynesia, and the strongly Malayan character of the flora, generally, of the Tonga, Fiji and Samoa Islands.

Several additional small contributions to the flora of the Solomon Islands have appeared (7), including some highly interesting novelties collected by the officers of H.M.S. *Penguin*, and the Rev. R. B. Comins. Excellent

photographs of the singular new genus *Sararanga* (Pandanaceæ) have been received at Kew, as well as ripe fruit in spirit, which will enable me to add to my published description, though not to complete it, because the male inflorescence is still unknown. Two species of *Begonia*, an *Oxymitra* (Anonaceæ) with flowers nearly nine inches long, a singular *Tabernæmontana* having a twisted fruit, and the anomalous genus *Lophopyxis* (8) are among the latest additions to the flora of the Solomon Islands. The last is doubtingly placed in the Euphorbiaceæ by Sir Joseph Hooker, and it has since been twice described (9 and 10), and placed in different natural orders, namely, *Combretopsis* (Olacineæ) and *Treubia* (Saxifragaceæ). There are two or three very closely allied species or races inhabiting Malacca, Ceram, New Guinea, and the Solomon Islands. I may refer in passing to a zoological paper (11) in which the author puts forward the theory of a former connection of the Solomon, Fiji, New Hebrides, Loyalty, New Caledonia, Norfolk and New Zealand Islands with New Guinea, but not with Australia. That there was, in the remote past, a greater land area in this region seems highly probable, but the relationships are so complex that fuller data are required to afford a solution of the problem. The present flora of Lord Howe Island, described a few pages forward, does not favour Mr. Hedley's views in their entirety on this point.

In my reference to the flora of Christmas Island (12) I overlooked a paper that supplemented mine to some extent (13), especially in relation to the vegetation.

Dr. Trimen (14) has published two more volumes of his admirable flora of Ceylon, bringing it down to the end of the Balanophoraceæ, following the arrangement of Bentham and Hooker's *Genera Plantarum*. The same author has drawn up a provisional list (15) of Maldive plants; the first, I believe, that has appeared. As might be expected there is no endemic element, and the vegetation is an assemblage of the ubiquitous coral island plants and weeds of cultivation. Dr. Trimen makes no mention of the *Cocos maldivica* or Coco-de-mer (*Lodoicea seychellarum*);

but, although it is improbable that this palm ever grew in the Maldive Islands, something yet remains to be done to complete its history. John de Barros, a Portuguese author, is thus quoted (16) by the writer of an article on these islands :—

"Their productions he also enumerates minutely, especially the coconut, both of the ordinary kind and of that called coco-de-mer, almost peculiar to the Seychelles, the seed of which appears to have been borne thence to the Maldivas by the currents of the ocean ".

Since the publication of my notes on the flora of New Zealand and the outlying islands (17) several interesting papers on the subject have appeared, though there is only one of sufficient importance to call for more than brief mention. But first the minor ones. Mr. F. Kirk is the author (18) of a series of monographs treating of the genera *Gentiana*, *Colobanthus*, and *Gunnera*, as represented in the New Zealand region, besides descriptions of a number of new species belonging to various natural orders. The forms of *Gentiana* are numerous, and the species exceedingly difficult of delimitation. Kirk defines ten species, and about half of them comprise several varieties. They are spread all over New Zealand, except the extreme north, and they extend to the Chatham, Antipodes, Auckland and Campbell Islands ; but hitherto no species has been found in Macquarie Island, the southernmost of these islands. They chiefly inhabit the mountains, in alpine and subalpine situations, and the sea-coast ; four out of the ten, it is stated, not being found out of the reach of the sea-spray. They all belong to one group, characterised by having pentamerous flowers, unappendaged corollas, and versatile anthers. White is the prevailing colour of all the species, though some of them occasionally exhibit various shades, mostly dull, of red, purple, and violet, and more rarely a pale yellow. This is in direct contrast to the behaviour of the northern species, speaking generally, and we are indebted to Mr. Kirk for the observation. *Colobanthus* (Caryophyllaceæ) is one of those densely tufted moss-like genera of which there are representatives in

various natural orders. It is one of the very few genera common to Australasia, to the Antarctic, and other southern islands, and the Andes, and confined to these regions. One species, *C. quitensis*, ranges from the mountains of Mexico to Cape Horn and reappears in New Zealand. Kirk also records it from Amsterdam Island, but that seems to involve two errors, for, so far as our data at Kew go, *C. diffusus* inhabits St. Paul, and no species is found in the neighbouring island of Amsterdam. One species, *C. Billardieri*, is found in the Alps of Victoria, in Tasmania, New Zealand, and the small islands southward to Macquarie. Two Falkland Islands species also recur in South Georgia, the southern insular limit of phanerogamic vegetation in the Patagonian region, if we except a grass, *Aira antarctica*, collected by Dr. Eights in the South Shetlands, about 62° S. lat., or 8° south of South Georgia. Kirk enumerates and describes ten species of *Colobanthus* from the New Zealand region, including four proposed new ones.

*Gunnera* (Haloragidaceæ) has a similar range to that of *Colobanthus*, save that it does not reach the colder limits either in America or the New Zealand region. Kirk brings up the species of the latter region to nine, four of which are new.

W. Colenso, D. Petrie, and H. C. Field also describe a few novelties (19), and the first named gives a charming description of his travels and botanising in the romantic country around Hawke's Bay, upwards of fifty years ago.

The one paper which I propose to discuss a little more in detail is devoted to the natural history of Macquarie Island (20), the most southerly speck of land in the New Zealand region known to support phanerogamic vegetation. It is in the same latitude (54° S.) as South Georgia in American waters, the flora of which I have described (21), where a list is given of the vascular plants inhabiting the island. They are separated from each other by about 164° of longitude, which in this latitude means, in round numbers, 5875 geographical miles; yet, as previously stated, nine out of thirteen of the vascular plants found in South

Georgia also occur in some of the southern islands in the New Zealand region. Later on I shall have something to say, or rather repeat, in explanation of this fact. It should be noted that these islands are in about the same latitude as York in England; yet the climate is now so severe in South Georgia and other conditions are so unfavourable to vegetation that the flora is perhaps poorer than in the highest northern latitudes yet explored, and entirely wanting the colour characteristic of many northern flowers. For example, such charmingly beautiful plants as *Papaver nudicaule*, *Silene acaulis*, *Saxifraga oppositifolia* and *Epilobium latifolium* are found north of the eightieth parallel; whereas the showiest flowers in South Georgia are those of a very small buttercup, so small indeed that they want finding. The flora of Macquarie Island is, however, not altogether devoid of colour, as witness *Pleurophyllum*; and *Stilbocarpa* is remarkable for its large rhubarb-like leaves.

Macquarie Island is between twenty and twenty-five miles long and five or six miles across in its broadest part. It is generally hilly, though the hills are nowhere above 800 feet. The following is a list of the vascular plants recorded by Mr. Hamilton (20), who visited the island early in 1894. I may mention that I had most of these plants under observation (22), and I do not agree in every instance with his and Mr. Kirk's (23) determinations; but the divergencies are unimportant; and there are several corrections of the names given in previously published lists. *Ranunculus crassipes*, *Cardamine hirsuta*, var. *corymbosa*, *Colobanthus muscoides*, *C. Billardieri*, *Stellaria decipiens*, *Montia fontana*, *Acæna Sanguisorbæ*, *A. adscendens*, *Callitriche antarctica*. *Epilobium nummularifolium*, *E. linnæoides*, *Azorella Selago*, *Stilbocarpa polaris*, *Coprosma repens*, *Cotula plumosa*, *Pleurophyllum Hookerii*, *Uncinia nervosa*, *Luzula crinita*, *Deschampsia Hookeri*, *D. penicillata*, *Poa foliosa*, *P. Hamiltonii*, *Agrostis antarctica*, *Festuca contracta*, *Aspidium aculeatum*, var. *vestitum* *Polypodium australe*, *Lomaria alpina* and *Lycopodium Billardieri*, var. *varium*. The last named one would have hardly expected to find in so high a latitude, where the

only woody plant is the small creeping *Coprosma repens*, because it usually grows on trees. A re-examination of a very small collection of Macquarie Island plants sent by Mr. Fraser of the Sydney Botanic Garden to the late Sir William Hooker, about sixty years ago, has led to the discovery of *Lycopodium Selago*, associated with *Azorella Selago*, a very similar plant in external appearance. In addition to the foregoing there are three colonised vascular plants, namely, *Stellaria media*, *Cerastium triviale* and *Poa annua*; and Mr. Hamilton states that he also collected *Tillæa muscosa* and two sedges, but the specimens were lost. If we except three imperfectly known grasses, which Mr. Kirk has described as new (24), there are no endemic plants in the island. The vascular cryptogams are all widely spread, two of them recurring in the northern hemisphere. Of the flowering plants upwards of half are confined to the New Zealand region, and the rest have a wider range. *Stilbocarpa polaris* (Araliaceæ) and *Pleurophyllum Hookerii* (Compositæ) are the two most remarkable and most conspicuous plants in this meagre flora; the former having large rhubarb-like leaves, and the latter silky, silvery leaves and handsome purple flower-heads in long racemes. *Colobanthus*, *Azorella*, *Acæna* and *Uncinia* are equally characteristic in the South American region.

Quite recently a fresh account of Lord Howe, Pitcairn and Norfolk Islands has appeared (25), but it contains nothing new on the botany of these islands. Special stress is laid on the beauty of the vegetation of Howe Island, where palms and tree ferns abound, and fig-trees of the banyan type attain dimensions hardly exceeded elsewhere. What is known, however, of the botany of this interesting island has appeared in Government Reports and scattered in a variety of publications (26-29) of limited circulation. It is true that Sir F. von Mueller long ago published (30) a bare list of all the plants known to him from the island, but it is incomplete, and supplies no information beyond the names of the plants. This being so, I am preparing a detailed account of the flora of this island with a view to publication elsewhere. I may here give,

however, some particulars gleaned from the publications referred to, though they are mostly anterior to the date (1885) to which I have limited myself generally in these articles, adding a few remarks of my own on the distribution of the plants.

Lord Howe Island is of small extent and peculiar conformation, situated about 300 miles from the coast of New South Wales in 31° 35′ S. lat. It is seven miles long with an average breadth of one mile, and the steep circular flat-topped elevations rise to a height of nearly 3000 feet. Norfolk Island, the nearest land to the north-east, is about 500 miles distant, and New Zealand, to the south-east, somewhat farther off. The island is of volcanic origin, consisting of three basaltic masses connected by coral-sand rock. About 165 species of indigenous flowering plants are known, and forty-eight ferns and lycopods. As already indicated palms form a conspicuous feature in the scenery. There are four species, all endemic, and they have been very much named, though three out of the four are well known under the generic name of *Kentia*. They are *K. Belmoreana*, *K. Canterburyana* and *K. Forsteriana*—names familiar to many persons, as they have long been favourite palms in cultivation on account of their elegance and hardiness. A tall and graceful specimen of *K. Forsteriana* is one of the finest ornaments of the central part of the palm-house at Kew. The fact of there being a good market for the seeds of these insular palms has led to considerable destruction of the trees to obtain them; but I believe the Government of New South Wales has made it a punishable offence to destroy trees on public territory. Beccari (31) has founded the genus *Howea* for them, which, if accepted, is the only endemic one. There are also four indigenous tree ferns, three of which are endemic. But the banyan trees (*Ficus columnaris*) are perhaps the most striking objects in the vegetation. Several appear in the photographs illustrating Wilson's Report, one of which is said to cover an area of three acres! *Moræa Robinsoniana* is an outlying gigantic member of an African genus of Irideæ very closely allied to *Iris* itself. It is known as the

wedding-flower, and there is a fine specimen of it at the south end of the cactus-house at Kew. *Carmichælia exul* (Leguminosæ) is the only species of a considerable genus, with this exception, not known to inhabit any other country* than New Zealand. There are other connections with the flora of the latter country, but they are mostly such as extend to Australia as well. *Pimelea longifolia* and the handsome sedge, *Gahnia xanthocarpa*, are apparently exceptions. In round numbers 25 per cent. of the species of flowering plants of Lord Howe Island are endemic, and 62 per cent. are common to Australia, many of these having a wider range. A few are common only to Australia, New Zealand, and Norfolk Island. The shrubby violaceous genus *Hymenanthera* is an example. The gum trees (*Eucalyptus*) of Australia are represented by the endemic *Acicalyptus Fullagari*, a small Fijian genus differing from *Eucalyptus* in having a calyptrate calyx-limb and separate petals. Two other conspicuous trees in the endemic element are *Dracophyllum Fitzgeraldii* (Epacrideæ) and the screw-pine, *Pandanus Forsteri*. The former is a tree, said to be the largest in the order, attaining the height of fifty to sixty feet. It has the foliage and aspect of a monocotyledon rather than of a dicotyledon. One characteristic Australasian type we miss in the Lord Howe Island flora, and that is *Cordyline*.

When reviewing (32) the newer literature relating to the flora of the Galapagos Islands I found little to add to what had been done by Darwin, Hooker and Andersson; merely mentioning the visit of the United States ship *Albatross*, and Dr. G. Baur's theory of the origin of the fauna and flora. Since then an account of Dr. Baur's botanical collections has been published (33), and the substance has also appeared in an English journal (34), and Dr. Baur himself has written (35) and lectured (36) in defence of his theory of the origin of this group of islands. As previously stated, he contends that the evidence points to the present condition being the result of subsidence; that the islands were formerly connected with each other and at a still earlier period with continental America. Although this theory has been derided, I think the biologi-

cal data strongly favour its correctness, and the soundings given in the map accompanying Agassiz's report (37) of the *Albatross* expedition show a relatively shallow area in which the Galapagos Islands are situated, and which extends eastward to the mainland of Veraguas. Probably the separation would be greatly anterior to the segregation of the West Indian Islands.

In the *Botany* of the *Challenger* expedition (38) I attempted a rough classification of islands in relation to the composition of their floras. These are defined as follows: 1, Vegetation comprising a large endemic element including distinct generic types; 2, vegetation comprising a small, chiefly endemic element, the derivation of which is easily traced; and 3, vegetation containing no endemic element. Without due consideration the Galapagos were referred to the first category. Sir Joseph Hooker (39) fully realised the absolute American affinities of the flora; but he analysed and discussed it as a derived one rather than as a remnant. Darwin, through some misinterpretation of the statistics supplied to him, fell into a singular error respecting the generic endemic element in the Galapagos (40). Referring to the Compositæ, he says: "There are twenty-one species, of which twenty are peculiar to this archipelago; these belong to twelve genera, and of these genera no less than ten are confined to the archipelago!" How this error arose it is impossible to say, but as a matter of fact the statement quoted is wrong (and was wrong at the time it was written) in all its details. With regard to assumed endemic genera of Compositæ, *five* were founded on galapageian plants, namely, *Microcoecia* and *Desmocephalum*, since reduced to *Elvira*; *Macræa* to *Lipochæta*; and *Scalesia* and *Lecocarpus* are so near to *Mirasolia* and *Melampodium* respectively that the late Mr. Bentham gave it as his opinion that they might well be reduced. Two genera from these islands belonging to other orders have also been reduced. These are *Galapogoa* = *Coldénia* (Boraginacæ), and *Dictyocalyx* = *Cacabus* (Solanaceæ); and *Pleuropetalum* (Amarantaceæ) has since been found in several localities in Western America. Taking this view of their affinities, there is not

a single genus of flowering plants endemic in the Galapagos; but each island has its distinct species. Briefly put then, the genera are the same in all the islands, and the genera are American; whereas a large proportion of the species are peculiar to each island, though they are not so exclusively confined to single islands as Darwin supposed. On this point he says (41): "Again *Euphorbia*, a mundane or widely distributed genus, has here eight species of which seven are confined to the archipelago, and not one found on any two islands. *Acalypha* and *Borreria*, both mundane genera, have respectively six and seven species, neither of which genera has the same species on two islands, except in the case of one species of *Borreria*." Dr. Baur's recent explorations necessitate a considerable modification of this statement; yet in a sense they confirm and emphasise it. Baur himself deals more particularly with the fauna (36) in illustration of this phenomenon. More than 400 specimens of the lizard genus *Tropidurus* were collected, and in the result he found that "each island possessed only a single species; all the individuals of an island belonged to one species; and nearly every island had its peculiar species or race".

The botanists who worked out Dr. Baur's collections selected *Euphorbia viminea* (33) as an example of a plant exhibiting racial differences in each of the eight islands, where it is now known to occur. The genera *Acalypha* and *Borreria* are cited as other instances. On the other hand, *Euphorbia articulata*, which was collected on four different islands, showed no such tendency.

In a former article in this journal (32) I mentioned the fact that huge branching Cactaceæ form one of the most striking features in the lower zone of the vegetation of the Galapagos, and I have elsewhere (42) given some particulars of what is known, and how little is known of these Cactaceæ; and I may repeat here that specimens of only one species have, so far as I can ascertain, been brought away from the islands. These were brought to this country by Darwin, and published by Henslow (43) under the name of *Opuntia galapageia*. This species is remarkable

in the genus for its very small flowers, which are only about three-quarters of an inch in diameter, and also for the small number of petals; but as the figure was made from dried specimens, it may be inaccurate in some details. In the same place it is mentioned that a species of *Cereus* was common in the island, but was not found in flower.

Darwin himself specially alludes (44) to the prominent feature these Cactaceæ are in the landscape, and likewise to the fact that they grow in the rough lava where there is absolutely no other phanerogamic vegetation. He further points out their importance as food for the gigantic tortoises and land lizards. They are also a source of water during the severe droughts, which often parch the lower zone.

Subsequent travellers have dwelt upon the part the Cactaceæ play in the biology of the island, and Andersson, a botanist who visited the islands in 1852, states (45) that he observed four or five species, but had time neither to prepare specimens nor sketch the plants.

My note on the subject in *Nature* came under Dr. Baur's notice, and he forwarded me two photographs, one representing a fine example of an arboreous *Opuntia* of great size, and the other a view embracing a number of large *Cerei*, together with a transcript of his notes on the subject in a paper (46) which I had not seen. He was struck by the difference in the appearance of the *Opuntiæ* on the different islands, and observed that the large *Opuntia* has a different habit on nearly every island. Thus, on Barrington, Indefatigable and South Albemarle, it develops a very tall stem; on Charles and Hood a relatively short but thicker stem; on Jervis a very short stem, branching from very near the ground, and on Tower Island it forms no stem at all, and appears as a dwarf bush. Dr. Baur attributes these modifications to the varying degrees of humidity, the greatest development occurring in the driest climate. In the lower region of South Albemarle, up to about 500 feet, the *Opuntia* is very common, attaining a large size, the largest being about twenty feet high, with a trunk two feet in diameter. " In old trees the bark looks

very much like that of a pine, and peels off in very thin sheets."

The common *Cereus*, which strongly resembles *C. peruvianus*, attains almost the same dimensions; but this is all we know about it at present, and there is clearly much more botanical work to be done in the Galapagos before the subject is exhausted. It may be of interest to add that no species of cactus inhabits the island of Juan Fernandez, but this may be ascribed to climatic differences. Indeed, so far as is known, none of the other Pacific American islands, at any considerable distance from the coast, support any members of the order, though Malpelo, for example, is barren enough to give them a chance of flourishing.

Another remarkable element in the flora of the Galapagos is the relatively large number of species of the small order Amarantaceæ. About fifteen species are now known to inhabit the islands, and twelve of them are endemic. They belong mainly to the genera *Telanthera*, *Alternanthera*, and *Froelichia*.

Concerning the flora of the Arctic Islands in relation to the adjacent continents, I have to add a few references (47-48) to works of older date than my paper (49), and a few recent ones of unusual interest. Mr. Trevor-Battye's account of the vegetation of Kolguev Island (50) and Colonel Feilden's contributions on the subject (51-52) rank first among these. The former noted ninety-five species of phanerogamia in Kolguev, and his observations on the vegetation are of great value. About a score of the plants recorded by Ruprecht (53) were not found, and Trevor-Battye remarks on the absence of *Saxifraga oppositifolia*, *Mertensia maritima* and *Ledum palustre*. Colonel Feilden's short paper on Spitzbergen plants, as well as his remarks on mild arctic climates, is worthy of attention on account of his experience. The only information I have found (54) respecting the vegetation of Einsamkeit Island is that there is no grass carpet, and it is added that there is a great quantity of drift-wood, sometimes far inland. A new list (55) of Iceland and Faeroe plants does not claim to be anything more than a contribution to local distribution.

There is little new literature relating to the Atlantic Islands, but Sir Joseph Hooker's comparison (56) of the Maroccan and Canarian floras was overlooked by me when reviewing the writings of Dr. Christ. In an article (57) of more recent publication, the latter gives expression to a considerable modification of his views on the affinities of the Canarian flora. He now recognises a much more intimate connection with the old African flora. But I must not reopen the subject here.

One important contribution (58) to the flora of the West Indies has appeared. This part consists of a critical elaboration of the Myrtaceæ, than which there was probably no group of plants more in need of revision. It is somewhat appalling to see such familiar trees as the allspice and clove with a page and half of synonyms each; yet it is very useful, historically, as well as for practical purposes, to have them brought together.

W. Botting Hemsley.

(*To be continued.*)

# THE PRESENT POSITION OF THE CELL-THEORY.

(CONCLUSION.)

THUS far I have tried to rehabilitate the cell as a vital unit. I have now to deal with the further question as to the part played by the cell in the composition of the higher animals and plants. In the earlier part of this essay I stated that Mr. Adam Sedgwick denied *in toto* the proposition that "the elementary parts of all tissues are composed of cells". Since writing those words, Mr. Sedgwick's reply to my previously published criticisms has appeared,[1] and I find that I have made a mistake. For he does not deny the proposition, but says: "The assertion that organisms present a constitution which may be described as cellular is not a theory at all; it is—having first agreed as to the meaning and use of the word cell—a statement of fact and no more a theory than is the assertion that sunlight is composed of all the colours of the spectrum". I can only beg Mr. Sedgwick's pardon. I certainly was led to suppose from his earlier writings that he regards the cell as a nonentity, in so far as it may be considered to be the ultimate structural unit of the metazoa, and I recoiled from his suggestion that the essence of development lay in "a multiplication of nuclei and a specialisation of tracts and vacuoles in a continuous mass of vacuolated protoplasm".

Mr. Sedgwick now explains that he objects, not to the statement that tissues are composed of cells—or, in his own words, that they have a composition which may be described as cellular—but to the statement that an individual metazoon is an aggregate of lesser individuals, or, as it has often been expressed, a cell colony or cell republic. I have elsewhere—and as Mr. Sedgwick well says, after great effort—come to agree with him on this point, for a careful survey of a considerable range of facts led me to the conviction

[1] Adam Sedgwick, "Further Remarks on the Cell-Theory, with a Reply to Mr. Bourne," *Quart. Jour. Micr. Sci.*, vol. xxxviii., p. 331, 1895.

that the idea of a cell republic was inappropriate. Such being the case I would willingly have buried the hatchet, but when I had already dug the hole to bury it in, my hand was stayed by some criticisms on his views and on mine which have just been published in a contemporary periodical.[1] These criticisms have restored to me the conviction which I held when I ventured to write a criticism of Mr. Sedgwick's views; a conviction that, as he originally expressed them, they were calculated to mislead and to do harm to the very cause whose interests he was desirous to promote. As he has lately explained that he did not mean what I supposed him to mean, there is no need for quarrelling any further with him, but he will himself allow that I was amply justified when I gave the following as a not unfair statement of his position. That from the connection known to exist between some cells composing adult tissues, there is an antecedent probability that similar connections exist between all cells composing all tissues; and this probability is heightened by observations made on the development of Peripatus, by the fact that the so-called mesenchyme cells in Avian and Selachian embryoes are continuous and not isolated as was once supposed, and by a study of the developing nerves of Elasmobranchs. And that it follows from this that the morphological concept of a cell so far from being of primary is altogether of secondary importance, and that progress in the knowledge of structure is impossible so long as men persistently regard cells as the fundamental structural units on which the phenomena manifested by organised beings depend. The true method of inquiry must be a study of the growth, extension, vacuolation and specialisation of the living substance protoplasm.

He has been understood by others as I understood him, and indeed he had so expressed himself that he could scarcely have been understood otherwise. What I had anticipated has happened. Persons, ready to grasp at novel ideas, have said in their hearts: "Tush, there is no cell! There are protoplasmic masses which may contain one or many nuclei; the mass is of no importance, it is scarcely

[1] *Natural Science*, vol. vii., No. 46, December, 1895.

more than the medium in which the nucleus lives, and through which it exhibits its powers. The nucleus may move about in the mass, acquiring 'spheres of influence' at its halting places, and so producing the vital phenomena. It is the nucleus which is the vital unit, and there is no bond between nucleus and cytoplasm which shall compel us to regard their union as the necessary condition of living individuality."

I have made use of my own expressions, but if this is not the plain meaning of the short editorial entitled "The Reign of the Nucleus" in the January number of *Natural Science*, what is?

The writer of the editorial is so captivated with the prospect opened up by his interpretation—a perfectly legitimate interpretation—of Mr. Sedgwick's writings, that he forthwith abolishes the existence of cells altogether and talks glibly of "protoplasmic masses," ignoring the fact that the masses in question are divided up into corpuscles. Following up his theme of protoplasmic masses dominated by nuclei, he lightly dismisses the arguments which I put forward, saying that the segmentations of Nereis, Unio, etc., exhibit nuclear lineage rather than cell lineage (who could hold such an opinion after a careful study of Wilson and Lillie's figures?), and winds up with the following astonishing piece of criticism: "In drawing an argument for the cell-theory from the definite places assigned to cells in development Bourne seems to us to have overlooked the experiments of Wilson, Driesch and Hertwig, who have shown that the nuclei may be moved about in the protoplasmic mass almost as freely as a 'heap of billiard balls may roll over each other'". I rubbed my eyes and wondered. I thought I knew the works of Driesch, Hertwig and Wilson pretty well, and that I had considered them carefully, and I had certainly regarded them as strong evidence in favour of the cell-theory as I conceived of it. A short search soon hit upon the passages which are professedly quoted. First for Driesch:[1] "Die Furchungs-

[1] Hans Driesch, *Entwicklungmechanischse Studien*, iv., *Zeitschrift für Wiss. Zoologie*, vol. lv., 1893.

kugeln der Echiniden als ein gleichartiges Material anzusehen sind, welches Man in beliebiger Weise, wie einen Haufen Kugeln durch einander werfen kann, ohne dass seine normale Entwicklungsfahigkeit darunter im Mindesten leidet". (The segmentation spheres of Echinids are to be regarded as a homogeneous material which one may roll amongst one another at will like a heap of balls, without thereby destroying in the least their capacity for development.) No hint whatever of rolling the nuclei through the protoplasmic mass. The statement is made of *Furchungskugeln*, that is of cells, and it is the cells that one may roll about like balls. Not a bad argument for my contention, that the blastomeres of many developing ova are disjunct. If there were any doubt as to Driesch's words a study of figures 39-68 which illustrate his paper would satisfy the most exacting. The blastomeres are unusually distinct from one another, especially in the embryoes illustrated by figs. 63 and 67. Now for Hertwig:[1] "Bei den verschiedenen Modificationen des Furchungsplasma werden die aus dem ersten Furchungskern durch aufeinanderfolgende Theilungen erzeugten Kerngenerationen Theilen des Dotters, die in Eiraum eine sehr verschiedene Lage einnehmen, zugetheilt und mit ihnem zu einem zellkörper verbunden. Die Kerne werden in Eiraum wie ein Haufen von Kugeln durch einander gewürfelt." This is a very complicated German sentence and might well lead to a misunderstanding, but it comes out all right in plain English. "In the various modifications of the divisional processes the nuclear generations, which are produced by successive divisions from the segmentation nucleus, are assigned to a portion of the yolk which occupies very different positions within the limits of the egg, and are bound with it to form a cell body. The nuclei are rolled one over another within the limits of the egg like a heap of balls." This passage is a summary of preceding statements and inferences, and it might be held to bear a very different meaning to that which it does bear; the illustration

[1] O. Hertwig, "Ueber den Werth der ersten Furchungszellen für die Organbildung der Embryo," *Arch. für Mikr. Anat.*, vol. xlii., p. 662, 1893.

of the heap of balls is a very loose one. To understand the meaning of the summary one must turn to pp. 678-685 of the same memoir, which consist of a section entitled "Erklarung des abnormen Furchungsverlaufes". There we learn, as we had previously learnt from Driesch, that the divisional planes of segmenting ova are determined by the direction of the nuclear spindles and that the orientation of the first nuclear spindle is determined by the character of the body of the ovum and its contents. The ova of Echinus are homogeneous throughout, and orientation of the first nuclear spindle is a chance affair. But the ovum of the Frog is not homogeneous; it consists of a smaller cap of protoplasm resting on a large body of yolk, and the nucleus lying in the cap of protoplasm, the direction of the first nuclear spindle is determined by its relations to the more active yolk on the one hand, and the denser food yolk on the other. The relations of the food yolk and protoplasm are changed by the pressure applied during the experiments and the changes are different according as the pressure is applied vertically or horizontally. Hence the direction of the first and the succeeding nuclear spindles is changed in different senses, according to the pressure employed. As the divisional planes are always at right angles to the nuclear spindles, the positions of the two first and the succeeding blastomeres differ according as the pressure applied is vertical, horizontal, oblique, or circumferential. One may in fact cause the blastomeres and their contained nuclei to take up what position one will by varying the direction of the pressure. In this sense, and in this sense only, can one speak of rolling the nuclei about like balls. Not a word about a protoplasmic mass through which the nuclei are caused to roll. On the contrary, a great deal about planes of division and splitting up of the egg into corpuscles round the nuclei. It only requires a glance at Hertwig's figures and diagrams to show that the blastomeres are as distinct during abnormal division as during normal division, and that there is not at any time any question of a "protoplasmic mass," a circumstance which has been well understood by everybody who has taken the trouble to read his memoir carefully.

Most of the experiments of Wilson, Hertwig and Driesch were of a different kind. They isolated the blastomeres by gentle shaking. Driesch is very careful to say *gentle*; rough shaking destroyed the individual blastomeres. Things which are so loosely united as to be separated thus easily from one another scarcely suggest the nature of a coherent protoplasmic mass.

The criticism falls entirely to the ground and one can only wonder how any one could have had the temerity to make it. The very objections urged to my views are but additional evidence in support of them, and I was well aware that the evidence existed when I wrote, but I had to be as brief as possible, and did not refer to it. My statement that it is very clearly established that there are numerous cases in which there is not "a primitive continuity which has never been broken" is abundantly justified. Mr. Sedgwick wonders why I emphasised the distinction and complete isolation of the cells formed by the segmentation of the egg. The reason is surely clear enough. Because he suggested, in no uncertain manner in his earlier writings, that the connections between adult cells were due to a primitive continuity which had never been broken, and that those who urged that such connections were secondary were in the wrong. This suggestion was contrary to fact, and it was my object to show that it was. I did not contradict myself when I stated immediately afterwards that the organism cannot be considered to consist of independent life units, for I went on to show that the cell-republic theory is also contrary to fact, and must therefore be condemned. If a contradiction exists, it exists in nature, and after we have ascertained the facts the next thing is to try to explain this seeming contradiction. Mr. Sedgwick says that he does not think it possible to do so, until we acquire some more understanding of the relative functions of nuclei and protoplasm. Possibly he is right, yet I think that an attempt may be made, and if the explanation is after all not very satisfactory yet some service may be done, for we may arrive at more distinct ideas about fundamental points, and we must gain much by a careful

classification of the facts. Such a classification has yet to be made. So long as a theory is dominant, as the cell-republic theory was, exceptions and difficulties are glossed over, or are explained away by a phrase. When I made a vigorous onslaught on Mr. Sedgwick, I was afraid that he wished to substitute King Stork for King Log and bring us under the domination of a new theory of his own. His reply to my strictures and his careful exposition of his own standpoint are reassuring on this point, and if I exceeded the limits of courtesy in my article, I did so under a misunderstanding and express my regret for it. Mr. Sedgwick has done a great service in breaking the bonds of the old theory. Now the question is, having got our liberty, what are we going to do with it?

Firstly, I think, we have got to make up our minds as to what we mean by a vital unit.

In the first part of this essay I stated that the cell is *par excellence* the vital unit, by which I meant nothing more than that it is the simplest form of material aggregate in which individual life is possible. There would seem to be no objection to such an application of the word unit. But the term unit is a relative one, and its correlative is multiple. If, therefore, we see that the developing embryoes of many animals and likewise the tissues of the adult forms are made up of structures which we must call cells, and if we call the cell a vital unit, we are obliged to conclude that the animals in question are composed of an aggregate of vital units, which leads us directly to the doctrine of a cell-republic. Thus at the outset we are confronted by the great difficulty that what experience teaches us to deny reason compels us to affirm.

There must be a flaw somewhere, either in the facts or in the reasoning. There can hardly be any doubt about the facts; the flaw therefore must be in the reasoning, and I do not doubt that it consists in our insistence on applying the idea of a unit to biological facts. As Whewell would have said, the idea is inappropriate. The term unit, as we use it in Biology, conveys a double meaning. On the one hand, it borrows part of its meaning from the idea of num-

ber, and to this extent the term is used in an equivalent sense to that in which it is used in Physics. But put side by side such expressions as unit of mass or unit of time with the expression unit of life, and a little reflection will suffice to show that the sense is inappropriate. Nor is the case made better if we compare the unit of life with the chemical unit. The value of the latter consists essentially in this, that it is a means of dealing numerically with chemical facts, and experience shows that ideas of number are very appropriate to chemical facts. With life the case is very different. In the present state of our knowledge the connection between life and number is of the slenderest kind, and it is insufficient to justify our applying numerical ideas to vital phenomena.

The other sense in which the term unit is used in Biology is purely subjective. It stands to express our idea of individuality, an idea which is founded on our own states of consciousness. It is unnecessary for me to dilate upon the controversies which have raged round this idea of individuality in its application to the animal kingdom. The most acute reasoners are not agreed upon the precise point where individuality ceases to belong to parts and belongs to the whole even in some of the simpler colonial organisms, and in such cases as the Siphonophora a satisfactory solution of the problem appears to be hopeless.

But these cases are simple in comparison with that which we are now discussing. If then we cannot agree about the limit of individuality in colonial organisms, how are we likely to agree about the same thing in the case of organic structure in general?

There is this to be said, however, that for us the test of individuality should be a biological test, and the idea is therefore more appropriate to the question than the numerical idea just spoken of. It was, no doubt, the recognition of its propriety which lent such force to Schwann's argument, "since it may be proved that some cells, which do not differ from the rest in their mode of growth, are developed independently, we must ascribe to all cells an independent vitality".

Hence, as it seems to me, whilst we can and ought to get rid of the numerical idea expressed by the word unit, we cannot get altogether rid of the idea of individuality, and we must do our best to bring it into harmony with the facts.

Since there is an inseparable connection between the idea of number and the word unit, we ought to get rid of the expression "unit of life," and use some other term which shall denote alike the simplest and the most complex of living beings. The word organism I have already objected to because of its double connotation—would it not be better to make use of such a word as "biont," which is as nearly as possible the equivalent of the German "Lebendiges"? Anything which leads or is capable of leading an independent individual life is a biont. Thus a cell may be a biont, as in the case of the protozoa, or it may be a constituent part of a biont, as in the case of the metazoa. In any case the cell is the simplest form of biont known, for if we go behind the cell we have structures which are not capable of leading an independent individual life.

But a cell in the case of metazoa, or the nucleus and other structures in the case of protozoa, and unicellular plants are things which, whilst they participate in, and contribute to life, and to that extent may be considered as living, are not in themselves capable of independent individual existence. They may be called metabionts.

The terminology suggested may not be perfect, but by the use of it or of something equivalent we may shake ourselves free of the false ideas which have clustered about individual life units, and start with a new hope on an inquiry into the nature and growth of bionts.

An essential part of our conception of a biont is the union of two substances, cytoplasm and nuclein. It does not matter, for present purposes, that we know nothing exact about these two substances, and still less of the manner in which they operate together to produce the phenomena of life. It suffices that we know that there are bionts whose structure is so simple that we can affirm nothing more of them than that they consist of cytoplasm and nuclein, *e.g.*, Bacteria, Yeast, Oscillaria, etc.

Within the limits of the protozoa we study many kinds of bionts which, whilst retaining great simplicity of structure, have advanced far beyond the stage represented by these simple forms.

The most important as well as the most striking structural advance is the formation of a nucleus. The nuclein which was, in the simplest bionts, distributed through the protoplasm, is aggregated to form a compact body, which from its structure and behaviour may be regarded as a metabiont, as also may the part from which it was segregated, the cytoplasm. The steps which lead up to the segregation of the nucleus are obscure, but there are very good grounds for saying that the nucleus, when formed, is connected, in some manner unknown to us, with the transmission of the so-called historic qualities of the biont. In any case it plays a leading part in reproduction, and the steps from the condition of diffused nuclein to centralised nuclein are suggested by the infusorian *Holosticha scutellum*, which ordinarily has no definite nucleus, but contains numerous chromatin particles scattered throughout its substance. Previous to reproduction by division the scattered particles are drawn together and unite to form a centralised nucleus, which divides in a normal manner and breaks up again into particles in the offspring.[1]

Besides the nucleus many other structural advances are to be noted in protozoa and in unicellular plants; some must be regarded as metabionts, *e.g.*, chlorophyll corpuscles and chromatophores of various kinds, many kinds of granules, etc. Other structures cannot be regarded as belonging to the same category, *e.g.*, cilia, contractile fibres, etc. We may for the present purpose leave both cases out of consideration, for it is the nucleus and the part it plays as an essential constituent of the biont which most concerns us.

We have as yet very obscure notions about the co-operation of nucleus and cytoplasm in the production of vital phenomena. But, putting aside the views of those who postulate the existence of minute vital units, and speak of

[1] Aug. Gruber, "Ueber vielkernige Protozoa," *Biol. Centralblatt*, iv., p. 170. See also the same author, *Zeit. für Wiss. Zool.*, xli., p. 186.

an emanation of specialised biophors from the nucleus into the cytoplasm, there is a general agreement that the co-operation is of the nature of a complex exchange of chemical material. If this be the case, the rate of exchange must be the measure of vital activity, and it is clear that the rate of exchange will be greatest in immediate proximity to the nucleus and will become increasingly less the greater the distance from the nucleus. At a certain distance, which might be called the limit of nuclear influence, the rate of exchange will be reduced to zero. We see that in the protozoa the forms which have a single nucleus are small, and we may say, in consequence of the foregoing considerations, that their size is determined by the limits of nuclear influence. But many protozoa are multinuclear, and I believe that there is no exception to the rule that protozoa of relatively large size are also multinuclear. Such is obviously the case in such forms as Radiolaria, Actinosphærium, Pelomyxa, the Myxomycetes and others. From a consideration of all the facts of the case we may legitimately infer that in any given biont growth beyond certain limits is incompatible with a uninuclear condition, and that further growth involves multiplication of the nucleus, which may have as consequences: (1) discontinuous growth, which in its simplest form is reproduction by binary fission: (2) continuous growth, in which the nucleus is multiplied so that all parts of the enlarged cytoplasm may receive an equal share of nuclear influence. There are numerous cases in which, as I pointed out before, the two conditions are combined. There is a cœnocytial[1] stage of considerable duration, followed by reproduction (or discontinuous growth).

The next phase is the formation of a biont of considerable size, in which very numerous nuclei are arranged in definite manner in a continuous mass of protoplasm. Such a condition is represented by the Cœloblastæ, and also in the

[1] When in my earlier essay I coined the word hypopolycytial I was not aware that Professor Vines had applied the term cœnocytial to the Cœloblastæ. His term has the priority and is more euphonious, so I adopt it instead of my own.

growing tissues of many animals and plants, as for instance in the embryoes of many Arthropods, in the endosperm of Phanerogams, etc. The condition may be permanent, as in the case of the Cœloblastæ, or non-permanent, as in the other cases. But in both instances there is a difference from the cœnocytial condition observed in Protozoa, namely, that the multiplication of the nucleus does not lead to reproduction in the form of the splitting up of the biont into as many new bionts as there are nuclei.

In a cœnocytial biont of appreciable size the relations of the various parts to external conditions will tend to become different, and differences of chemical constitution will be set up in the different regions exposed to different conditions. We can see that this is the case in Botrydium, in which root and shoot are plainly marked off from one another, and better still in Caulerpa, in Codium, and in many of the Moulds. Differences in chemical constitution thus induced will mean difference in exchange between nucleus and cytoplasm, and we may infer that, in accordance with these differences, the cytoplasm within the limit of influence of any one nucleus will in time assume a constitution so different from that of the adjacent cytoplasm as to become sharply marked off from it. It will then acquire its own surface tension—the first step towards a cell wall—and will be a separate corpuscle containing a nucleus, in fact a cell. Such a cell however has not come into being as an individual unit joined to its like, either phylogenetically or ontogenetically, but it has from the first formed a part of an organic whole, of which it is nothing more than a specialised component part.

One looks naturally for evidence of this mode of formation of cellular structure in developing Metazoa. The best evidence is to be found, I think, in the segmentation and formation of the layers in many Cœlenterata. In some Cœlenterata—for example, in Renilla—the nucleus divides without accompanying division of the cytoplasm until eight or sixteen nuclei are present, and then the cytoplasm divides and eight or sixteen cells are formed. But of more importance than this is the formation of the layers. From

the considerations stated above we should expect that the changes in chemical composition of the cytoplasm and the correlated changes in the nucleus, in other words the differentiation, would first become manifest in the peripheral parts of the growing cœnocyte, and that we should have a stage in which there was a cellular external layer and a cœnocytial internal mass. We find that in fact in the embryoes of many Cœlenterates the outer layer is divided up early into sharply defined cells at an early period, whilst the central cells retain the character of a cœnocytium; at most the cell outlines of the internal mass are confused and indistinct.

We see also that in the growing tissues of the embryoes of higher animals the embryonic tissue is not cellular but is a cœnocytium, for example, the mesoblast of Avian and Selachian embryoes and of the Rabbit. It is only at a later stage when different relations to other parts of the body have been acquired and new exchanges of material are forced upon the growing mass, that the continuous mass of cytoplasm is split up into corpuscles, each of which, in my view, corresponds to the limit of influence of a nucleus.

On the other hand we have the undoubted fact that in many organisms there is no cœnocytial phase in development, but the cytoplasm surrounding the nuclei as they are successively formed is immediately marked off into definite corpuscles, so that the whole process of development suggests the formation of an aggregate of bionts derived by division from a single parental biont. An explanation of this fact presents many difficulties, and I have not now the space to discuss these difficulties and to show that, obscure as the subject still is, there is ground for supposing that the difficulties are chiefly due to the prepossession which exists in most minds in favour of the independent life unit theory. I hinted in my previous paper (*loc. cit.*, p. 171) that the discrete condition of the blastomeres of so many embryoes may be connected with the fact that they are, from the very outset, specialised. This means that as the nucleus is in some way associated with the transmission of historic qualities, these qualities may be located in special

parts of the nucleus, and on division, some of the daughter nuclei may possess one set, others may possess another set of "qualities". By "qualities" I conceive that we mean different chemical constitutions, and it would follow that the daughter nuclei, being of diverse chemical constitutions, would react in diverse manners on the adjacent protoplasm and would each cause the delimitation of a territory of cytoplasm within the limits of its own sphere of influence; in other words, cell bodies would be formed round nuclei of different chemical constitutions.

There is, however, yet another consideration to be taken into account. As Hertwig has shown, the cytoplasm in many ova is not homogeneous but is obviously separable into tracts of unquestionably different chemical constitution. This is conspicuously evident in the ova of Amphibia. As the nucleus divides, its products come into relation with different kinds of cytoplasm and the exchanges between nucleus and cytoplasm will be different in different places within the limits of the egg. Arguing on the same principles as before, we may attribute the successive formation of discrete blastomeres to this factor as much as to the separation in the course of division of different qualities contained in the egg nucleus, and according as one leans towards an epigenetic or an evolutionary theory of development so will one be disposed to lay more stress on the one factor or the other. There is this much to be said, that the most remarkable cell-lineages (which are only traceable when the blastomeres are discrete) have been observed in ova which contain a considerable proportion of yolk, which is not evenly distributed throughout the egg, and it is suggestive that segmentation in all cases leads to the segregation of corpuscles richer in yolk from corpuscles poorer in yolk—in fact to the segregation of materials of diverse chemical constitution.

Tempting as it is to pursue this subject further, I must not attempt to do it now. But as I have claimed that the views which I have tentatively put forward are agreeable to the facts which we are in possession of, I may well give a short summary of the facts which I have relied upon.

(1) The co-existence of two substances at least, nuclein and cytoplasm, is requisite for life. (This is an inference, strictly speaking, and not a fact; but I think that it may be considered a legitimate inference from what we know of the structure of the lowest bionts, and from the experiments of Nussbaum, Gruber, Verworn and others.)

(2) The existence of bionts, such as Bacteria, in which we are unable to distinguish more than these two substances. (This is a fact, which lends material support to the above inference.)

(3) The existence of bionts in which nuclein and cytoplasm are not indefinitely intermingled, but the former is segregated in the form of particles scattered through the protoplasm, *e.g.*, *Trachelocerca phœnicopterus* and *Chœnia teres*. (We gather from this fact that the two chemical substances tend to become separated from one another.)

(4) The temporary aggregation of nuclein particles to form a centralised nucleus for the purpose of the reproductive act, *e.g.*, *Holosticha scutellum*. (We infer from this that there is some connection, at present hidden from us, between the nucleus and the reproductive act.)

(5) The existence of many bionts in which the nuclein is concentrated to form a nucleus. (We infer that this is a grade of permanent differentiation arising out of the previous temporary grade.)

(6) The existence of many nuclei in all bionts which, whilst still undivided as regards their cytoplasm, attain to a certain size. (From this we infer that the "limit of nuclear influence" cannot extend through a large mass of cytoplasm.)

(7) The origin of "cellular" tissues from a cœnocytial mass, *e.g.*, the endosperm of Phanerogams; the neural crest of certain Vertebrate embryoes; the embryoes of Arthropods; the mesoblast of many Vertebrates, etc. (From this we infer that the cells composing many tissues of higher animals are not to be regarded as bionts, but are secondarily derived during the growth and extension of the parts of a single biont.)

This *résumé* suffices I think to show that this at least

may be claimed for the views which I have put forward. They are founded strictly on the facts, and they do not depend on the assumption of any kind of hypothetical units of which the nature and even the very existence is entirely beyond our ken.

Since I have not been able to develop my views, I cannot but expect that they will be subject to considerable modification and even to entire overthrow. They form at least an attempt to classify and colligate the various phenomena which seem to be germane to the subject, and I have collected and compared a much larger body of facts than I am here able to refer to, without finding any which are contradictory to my ideas. That my ideas are somewhat indistinct need not, at present, be urged as an objection, for indistinctness is not necessarily a sign of falsity. The cell-republic theory was not wanting in distinctness, but it was inappropriate to the facts. I only claim that my ideas are appropriate, and I shall hope to give them more distinctness on a future occasion.

In the meantime I leave out of consideration a large question, concerning which I think it scarcely possible to give a satisfactory account, in this standing in opposition to Mr. Sedgwick, who thinks that which I have attempted to be impossible, but offers a solution of that which I think scarcely possible.

The question is, how are we to account for that phenomenon which I have described as a progress from the state of an independent corpuscle, through a state of many coherent or continuous or conjunct interdependent corpuscles, back again to the state of a single independent corpuscle?

Mr. Sedgwick's solution is this: that the unicellular form is assumed by metazoa in order that conjugation may be possible. The single independent corpuscle which recurs in the cycle is the sexual cell, and the essential feature of sexual reproduction is the conjugation of reproductive cells. The unicellular phase is only assumed in sexual, not in asexual reproduction, and this is to be explained by the consideration that conjugation is as necessary in metazoan life as in protozoan life, but that conjugation between the

ordinary forms of metazoa is impossible for mechanical reasons, and therefore special individuals of a form simple enough to admit of conjugation are produced. These special individuals are the ovum and spermatozoon.

The explanation is extremely ingenious and there is nothing unreasonable in it, but one cannot say that it is altogether acceptable at first sight. It would have been more satisfying if Mr. Sedgwick had marshalled some of the facts relative to the sexual reproduction of some of the lowest multicellular organisms and had shown their relation to his suggestion. A difficulty which at once occurs to me is that in many plants asexual reproduction is effected through the agency of a single cell. In fact, before one can accept any solution of the question one requires a very extensive and careful survey of all the facts known about the reproduction of the lower plants. They afford examples of every conceivable grade of the reproductive processes, and, once one begins to look into the subject, hints as to the parting of the ways of sexual and asexual reproduction occur to one at every step. The pity is that the mere zoologist, who does not find such a fruitful field in his own territory, is obliged to disinter the facts from the load which the peculiarities of botanical terminology have heaped upon them.

It is quite possible, however, that such a survey would afford strong support to Mr. Sedgwick's opinions, and if it should do so they would in no way be inconsistent with the ideas which I have put forward, but would rather support them.

A word in conclusion for those who will reproach me for having attempted to frame a chemico-physical theory of organic growth, and for having used such phrases as "complex chemical constitution," "exchange of chemical material," etc., without assigning any distinct meaning to them. I admit that our knowledge on the subject is rather inadequate, and that I have used obscure phrases to express relations which are in themselves obscure. If one attempts to lift the veil of obscurity one must inevitably

call hypothesis to aid, and it has been my object to avoid the use of hypothesis where I could do without it. It is, however, legitimate to frame an argument which, while it agrees with the lessons of experience, is ultimately based upon hypothetical considerations, provided always that those considerations are consistent with the accepted teaching of the sciences whose aid is invoked.

Any attempt whatever to find an explanation of vital phenomena ends in an appeal to chemistry and physics. Knowing as we do that the elements of which organic bodies are composed are not different from those which occur in the inorganic world, we cannot refuse to acknowledge that vital processes are in the end chemico-physical processes, and this much is conceded by every author of a theory of vital units. The difficulty which they have to face is the same as that which I have to face, and is not one whit the less because it is compressed into the limits of a biophor, whereas I would allow it the limits of a cell. Can we frame any distinct ideas of these chemico-physical processes? Not very distinct ideas, perhaps, yet we can supplement the lack of positive evidence by analogies and illustrations involving the same ideas as those which are current in the physical world.

It was Professor W. K. Clifford, I think, who first drew a graphic picture of the molecular forces which are at work in any chemical compound, by describing the atoms as linked to one another and dancing a sort of merry-go-round within circumscribed limits. We may carry on the illustration, which, fanciful though it may seem, is supported by physical and mathematical considerations. A biont is a great organised war dance, performed by a whole army corps. The individuals composing each company are the atoms, they are linked to one another by companies and each company dances its own figure. Every company is a molecule, and every company dance is but a part of a larger dance, in which the companies act in relation to one another as the individuals act in the company dance. The larger dances are regimental dances and every regiment is a micella. The regimental dances are but parts of still larger

brigade dances, and the brigade dances are but part of the great dance of the whole army corps, which, taken as a whole, is the biont. The illustration is not quite exact, for each company must not be considered as consisting of like individuals, but of many individuals of all arms, some like and some unlike, linked in such various ways that no two companies are the same, partly because of the proportions of different kinds of individuals composing them, partly because of the way in which those individuals are linked together. Nor must we imagine that individuals are permanently attached to companies, nor yet companies to regiments, but that in the course of the dance individuals are passed from company to company, and companies from regiment to regiment, each conforming temporarily to the particular figure of that part of the dance to which he or it for the time belongs. Further than this the individuals engaged in the whole dance are never long the same: there are bystanders who for a time do not participate in the dance but are caught up one by one, whirled through the figures, passed from company to company, from regiment to regiment and brigade to brigade, and are eventually passed out of the dance again, after having participated in some or all of the figures as the case may be. Every individual in the dance is at some time passed out of the dance, becomes a bystander, and may again be caught up and whirled along in the dance once more.

The illustration is fanciful, if you please, but it is of the same kind as illustrations used to depict the play of molecular forces in the inorganic world. It serves a purpose in that it gives the imagination something to work upon, and it enables one to conceive of the immense complexity which is possible in a chemico-physical process. The army dance which I describe is capable of any number of combinations, a number amply sufficient to satisfy the needs of those who insist so strongly on the marvellous complexity of life. Let anybody imagine an army to be composed of four brigades, each brigade of four regiments, each regiment of ten companies, and each company to contain 100 individuals of the eight kinds, carbon, oxygen, hydrogen,

nitrogen, sulphur, phosphorus, potassium and iron, in varying proportions, and let him work out the possible combinations. I think he will be satisfied with the complexity.

What then of heredity and of the capacity which I have mentioned for acquiring historic qualities?

Believing as I do that the vital processes must in the end be attributed to a particular mode of molecular motion, I believe that it is the form of movement which is transmitted. Returning to my illustration I would say that it is the figure of the whole dance which makes up the species, and that it is the figure—the mode of motion—which is inherited, clearly not the individuals engaged in the dance, except in a very small degree, for they are constantly coming into the dance anew and as constantly being passed out of it. Under certain circumstances there may be an excess of one or more kinds of new individuals pressing into one part of the dance which will affect the figure of the company dance which they crowd into, and this will affect regimental figures and ultimately, in decreasing degrees, the whole army figure. In this way we may picture to ourselves the action of external influences in bringing about variation. But I have given rein enough to my imagination. The picture was introduced partly to show that beneath my obscure phrases there was some distinctness of ideas, partly to emphasise the immense complexity of Nature and to show that even atoms and molecules may be conceived to be so combined together that, in Goethe's words, " sie bewirken so eine unendliche Production auf alle Weise und nach allen Seiten ".

GILBERT C. BOURNE.

# THE HEREDITARY TRANSMISSION OF MICRO-ORGANISMS.

IT is well known that in the construction of many of the theories of heredity the doctrine of the transmission of acquired characters has obtained considerable prominence. The hypothesis of Lamarck rendered it necessary to assume that structural characters which had arisen from the use or disuse of organs, became an integral part of the individual and reappeared in the descendants, and although the application of this idea became greatly restricted when the principle of natural selection was established, it is only within the last few years that the transmission of acquired characters has been considered as unproven, and the instances put forward in support of this view to be capable of a different explanation. It may be admitted that mutilations and permanent injuries can be included among acquired characters, and the structural and functional modifications of the individual which occur in disease may persist, and therefore also be considered as definite morphological or physiological changes. Mutilations apparently do not pass from parent to offspring, and this has been especially pointed out by Weismann and his followers, since, if heredity is capable of explanation on the hypothesis of the continuity of germ-plasm contained in definite reproductive cells, any change in the structure or modes of activity of the essential body or somatic cells would not be transmitted. An identical line of argument also negatives the belief that diseases can be inherited, and this view was maintained by Weismann in his well-known criticism on the transmission of experimental epilepsy; the symptoms in this hereditary disease he considered might be due to some unknown microbe which found its nutritive medium in the nervous tissues and was transmitted in the reproductive cells. The question whether micro-organisms can actually pass from parent to offspring is one which has been carefully investigated, whereas

the proof that actual morphological changes, such as modifications of histological or molecular structure, can be transmitted has not yet been given. It is conceivable that predispositions may be inherited, and these must result from alterations in the germ-plasm, or a direct infection of the germ or embryo might cause the transference of a disease from one generation to another, a phenomenon which simply depends upon a particular mode of conveyance of a parasite.[1]

In many diseases, and particularly those which are directly caused by micro-organisms, it is a matter of interest to note the wide differences which exist between the conveyance of hereditary characters, and of a specific disease. Armauer Hansen (1) has made this perfectly clear in considering the etiology of leprosy. He has pointed out that true hereditary characters are usually limited to one sex, frequently appear at a particular age, and the phenomenon of atavism is not rare; but in the conveyance of such a disease as tuberculosis or leprosy, none of these conditions are fulfilled. It is a logical deduction from the consideration of these differences that every specific disease which is transmitted cannot be regarded as hereditary, but as an instance of the direct bacterial infection of the germ-cells or embryo. Most writers on cancer and malignant growths have discussed the hereditary transmission of this disease, and if it is allowed that a disposition to cancer may be derived by inheritance, then this condition would depend upon some peculiarity inherent in the nucleus of the germ-cells; but if, on the other hand, malignant disease is caused by a parasite belonging, as some investigators have sought to prove, to the group of protozoa or protophyta, then the transmission of the actual disease will depend upon the passage of a micro-organism which invades the germ or its

[1] "Pour les maladies, vraiement constitutionnelles, c'est la substance héréditaire elle-même qui est viceuse; pour les maladies infectieuses, le vice n'est pas dans la substance elle-même, mais à côté d'elle, et les produits sexuels servent seulement de vehicule à un parasite capable d'engendrer plus tard une maladie générale." Y. Delage, *La Structure du Protoplasma et les Theories sur l'Hérédité.* Paris, 1895.

product, and the whole phenomenon ceases to be one of heredity, for the hereditary transmission of micro-organisms is simply a particular instance of bacterial infection. The inheritance of actual specific disease must therefore always be considered as a problem absolutely distinct from that of heredity and incapable of explanation by any hypothesis of heredity.

Micro-organisms which reach an individual either by inheritance or other modes of conveyance may undoubtedly exhibit a period of latent life extending over many years; but when this condition is succeeded by an active life, to establish the proof of an hereditary transmission is exceedingly difficult or even impossible (11). The early researches into problems of this nature were necessarily made with the help of statistical and clinical methods; but it is the application of experimental methods, which could only be pursued with success as the study of bacteriology developed, that has finally succeeded in removing the subject of the hereditary transmission of specific diseases from the hazy region of speculation. The attitude assumed by Baumgarten and his followers on this question is well known. In the case of tuberculosis it is maintained that individuals are rarely infected with tubercle bacilli after birth, but that the disease in the majority of cases is due to a parasitic infection of the egg-cell or embryo. It is even urged that the bacilli may remain latent in one individual, and only enter upon a phase of activity in the offspring, a view which, if correct, would accord with the opinion of many clinical observers. While destroying the opinion so commonly held that an "inherited tubercular predisposition" exists, Baumgarten's theory of hereditary parasitism makes a still greater demand on the imagination; but that the views of this distinguished pathologist have acted as a stimulus to renewed experimental work on the transmission of micro-organisms is beyond doubt. Recent papers by O. Lubarsch (2) of Rostock and J. Csokor (3) of Vienna give an admirable exposition of the present position of our knowledge on this subject of the transference of bacteria from parent to offspring in man and

the lower animals, and the evidence that bacteria may in this manner gain access to the organism is incontestable.

In inherited specific diseases it is possible to distinguish two forms of infection : first, by a direct bacterial invasion of the essential reproductive cells ; secondly, the egg-cell or the embryo may receive micro-organisms from the female, in which case the blood stream is the channel for conveyance, and the whole phenomenon is then one of metastasis comparable in every respect to what obtains when bacteria multiply at a definite area of the body, and thence become distributed by the blood and lymph in distant parts of the organism. Bacterial infection may therefore be either germinative or placental, and in mammals the latter form of transmission is not infrequently observed. The specific bacteria of anthrax, typhoid fever (6), pneumonia and tuberculosis (7) have been isolated from the human fœtus, cultivated, and successfully inoculated upon animals, so that the chain of evidence is complete. The pyogenic cocci such as streptococcus pyogenes (24) and staphylococcus pyogenes aureus have also been demonstrated in fœtal tissues by Fraenkel and Kiderlen, and Auché has shown that in small-pox the placenta may be penetrated by these micro-organisms. In the lower animals not only may the bacteria already mentioned be transmitted, but also those of cholera, glanders and chicken cholera.

In many animals the egg-cell is the largest unit of the organism, and would be capable of containing numberless bacteria ; that such an infection does occur was first established by the classical observations of Pasteur (4), which have been confirmed by all subsequent investigators. In pébrine, a disease of silk-worms, definite sporocyst forms (microsporidia or Cornalia's corpuscles) are transmitted from the imago in the egg-cell, and the larva is directly infected in this manner. Blochmann (5) has also described a similar mode of conveyance of bacteria in the ova of *Blatta orientalis*. In a single instance a tubercle bacillus has been seen in the mammalian ovum. The sperm-mother-cells may also be invaded by micro-organisms, but this is rare, and no example of an infected male reproductive cell

exists. That this condition will ever be demonstrated is improbable, since bacteria contrast with parasitic protozoa in infecting the cell and sparing the cell-nucleus, and the essential agent in the process of fertilisation is the nucleus or head of the sperm-cell.

Various observers have attempted a solution of this question of germinative infection by the employment of two different methods. The first of these is that pursued by Maffucci, who directly infected the fertilised eggs of the fowl, and in the second not only were the genital glands and the products of these examined for micro-organisms, but pieces of them were taken from animals suffering with specific diseases and used as material for inoculation.

Even if it is assumed that an ovum actually is a site in which bacilli such as those of tuberculosis exist, it may be objected either that the microbe is dead, or that such a cell is incapable of development. This is the attitude taken by Virchow, who absolutely denies the existence of congenital tuberculosis. Maffucci's experiments, however, contradict this opinion, for this observer has shown that the bacilli of avian tuberculosis develop in an infected embryo, and the chicken succumbs to tuberculosis in twenty days to four and a half months after hatching. If, however, instead of infecting the embryo, bacteria such as those of chicken cholera, or anthrax, or Friedländer's pneumococcus are introduced in the extra-embryonic area, then these organisms may actually enter the embryo through the allantois but do not increase in number provided the embryo remains alive. The pathogenic micro-organisms may therefore be destroyed or attenuated by actively proliferating embryonic tissue cells, or they may become capable of development at a later period of life, in other words, remain latent. Although these experiments were devised to establish the view that a genuine germinative infection may occur, they obviously do nothing of the kind, and it is to the researches of Gärtner that we owe an absolute demonstration that ova may contain pathogenic germs. Gärtner among other animals inoculated canaries with mammalian tubercle bacilli. After a few weeks he removed

nine eggs, washed these in dilute corrosive sublimate, dried them in wool and introduced the contents of each egg into the peritoneal cavity of guinea-pigs. In two cases tuberculosis was set up, the animals dying one and a half months and two and a half months after infection. These experiments, which are absolutely free from objection, conclusively prove that the egg-cell may contain virulent bacteria, and it is easily conceivable that such eggs may develop and the transmission of the parasite take place by direct germinative infection, especially since Maffucci's work shows that such infected eggs are capable of development.

Jani, Westermayer, Spano, Walther, Gärtner, and quite recently Jakh, have microscopically investigated the bacterial contents of the reproductive glands, and also inoculated animals with fragments of these organs. With the exception of Gärtner's researches these experiments have not added greatly to our knowledge of the hereditary transmission of bacteria. All the experiments of Westermayer were negative. In fourteen cases of well-marked general tuberculosis no tubercle bacilli could be recognised, and inoculation experiments were failures. The experiments of Jakh (10) were more fortunate. Five inoculations with pieces of the male reproductive gland and its product, taken from individuals dead of tuberculosis, gave three positive results. If the gland alone was used, the experiments were always negative, and of three inoculations with pieces of the egg-forming gland one was successful. It may be admitted that these experiments do not really throw much light on the subject of germinative infection, but Gärtner's researches are of much greater value. He experimented upon mice, guinea-pigs, rabbits, and canaries, these birds being susceptible to mammalian tubercle bacilli. Having inoculated these animals with bacillus tuberculosis, a careful examination was made of the offspring of such tubercular parents. This method might naturally be expected to give a conclusive answer to the question of hereditary infection, and the following information has been gained from these researches:
1. The sperm rarely contains tubercle bacilli—five in

thirty-two cases. Even if micro-organisms exist they are incapable of infecting the egg. In twenty-two (rabbits) and twenty-one cases (guinea-pigs) where the male reproductive gland was the seat of an acute tubercular process, the offspring were never infected. 2. Neither does the male infect the female by way of the sperm. 3. Infection takes place frequently from the female to the fœtus, and in an overwhelming majority of cases by way of the placenta.

A few considerations may make the importance of Gärtner's work more evident. If bacilli exist, as they occasionally do, in the product of the male gland it is probable that this material, like other parts of the body, contains bacteria only a few days before death, for we know that quite an abnormal number of micro-organisms may invade the whole organism during the last days of life. Tubercle bacilli are immotile and therefore will not easily reach the oviduct or egg, a matter of some importance, since it has been shown that in most cases the ovum is fertilised either high up in the oviduct or even at the time of liberation from the Graafian follicle. Stroganoff (12) has also pointed out that the uterine area is sterile, and the secretion of this is bactericidal, in which it resembles sputum (13) or the mucus of the nasal tract which is almost free from germs (14). Lastly, it is well known that a single male morphological unit is sufficient for fertilisation, and if we assume with Gärtner that 100 virulent tubercle bacilli are mixed with sperm-cells, the ratio of bacteria to these would be about 1 : 22,500,000; it is hardly conceivable on the doctrine of probabilities that a bacillus would gain access to the egg. It may therefore be considered, both on experimental and theoretical grounds, that a germinative infection of the ovum never occurs by the conveyance of micro-organisms in the male reproductive cells.

The difficulties which exist in proving that the inheritance of a specific disease may occur through an infection of the ovum are fortunately not so great in those cases where the passage of micro-organisms takes place solely from the female to the fœtus by way of the placenta. It is established that specific micro-organisms can pass by this

route. It is not even necessary to assume that there is any lesion whatever in the placenta or that the epithelium of the fœtal villi is destroyed. An experiment by Zuntz shows clearly that particulate material will easily pass into the amniotic fluid from the maternal portion of the placenta, for if indigo-carmine is injected into the veins of the female the dye passes into the amnion leaving the fœtus free, and in this very manner anthrax bacilli may pass, and from the amnion gain access to the mouth of the fœtus, enter the gut and set up disease by a primary infection of the wall of the intestine (25). An intra-uterine infection, therefore, can occur without lesion of the placenta, though in the majority of cases this structure is primarily infected, and then the fœtus, or else hæmorrhages of the placenta permit the passage of micro-organisms. However the undoubted fact that micro-organisms can penetrate the healthy skin by way of the hair follicles—and the same is possibly true for the epithelium of the urinary tract—must not be forgotten in considering the passage of bacteria across the placenta. This structure may be normal and even then allow the transit of bacteria. Birch-Hirschfeld (15) from researches on the structure of the human placenta as well as that of mice, rabbits and goats considers that the bacilli of anthrax at any rate can traverse the uninjured chorionic epithelium. Moreover in the human placenta and in rabbits numerous processes of the chorion traverse the placental sinuses, and these processes are normally destitute of epithelium. It was noticed by Max Wolff (16) that anthrax bacilli easily pass if the placenta was crushed or torn, and micro-organisms which exert a necrotic influence on tissues, such as the pyogenic cocci, appear first to destroy the epithelium of the chorionic villi, and then pass through into the fœtal blood. In this fluid micro-organisms reach the liver, and it is this organ which, as a rule, is primarily affected, and then the glands in the lymphatics leading from the organ become implicated. The location, therefore, of tubercles in fœtal tuberculosis is characteristic, and all observers insist upon this feature in determining whether tubercular

deposits are of intra- or extra-uterine origin in early cases of the disease. As a matter of interest it may be mentioned that quite recently Bar and Rénon have demonstrated tubercle bacilli in the blood of the umbilical vein (7). The method used by these observers, that of inoculating guinea-pigs with the suspected blood, and in this manner establishing tuberculosis, is not so convincing as the actual demonstration of bacteria in fœtal tissues. Wassermann (17) in a recent paper especially insists on this point, and discards all evidence of inherited disease which rests simply upon inoculation experiments. He describes a case of early tuberculosis which ended fatally when the child was ten weeks old, where the disease was acquired, not from the parents who were healthy, but by direct infection from a tubercular relation, and believes that such cases as these are not infrequently cited as instances of congenital disease. In his opinion hereditary transmission of bacteria does occur, but it is exceedingly rare in comparison with the frequency of extra-uterine infection. Bernheim (18) considers that the offspring rarely, if ever, become tubercular if separated from tubercular parents, with the exception of those cases where the placenta is infected. The case reported by Ivan Honl (19) of a child fifteen days old that on autopsy showed tubercular nodules in the liver, spleen, and lungs, and numerous bacilli, must be classed as a definite case of transmission which with many others lends no support to Eberth's statement that individuals do not inherit tuberculosis but acquire it (23).

A recent case of congenital typhoid fever is related by Freund and Levy (20), and instances of transmitted hæmorrhagic infection have been recorded by Neumann (21) and by Dungern (22). The numerous examples which the journals of veterinary science contain, especially the work of Bang, Kockel, and Lungwitz, also afford conclusive evidence of the transmission of pathogenic micro-organisms, though there is a consensus of opinion that the placental is far more frequent than the germinative infection. The share borne by the male in this transmission may be disregarded, as no bacteriological evidence exists to support

this view. Finally, the frequency of hereditary transmission of pathogenic germs is exceedingly small compared to other modes of infection.

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## PREHISTORIC MAN IN THE EASTERN MEDITERRANEAN.

THE purpose of these notes is to summarise the results of recent research among the prehistoric peoples and civilisation of the Eastern Mediterranean; especially in so far as these prepare the environment for the first great civilisation of Europe, namely, that of Greece, and fill the chronological gap, and explain such communication as existed, between this and the equally "historic" but far earlier civilisations of the Euphrates and Nile Valleys.

A strictly "Historic" Age on the shores of the Ægean Sea, or in fact in the Eastern Mediterranean at all, cannot be said to begin before the seventh or at earliest the end of the eighth century B.C.; and everything before this point would certainly have been classed as "Prehistoric," but for the fact that, until quite lately, the preceding centuries have been interpreted wholly in the light of a voluminous Greek tradition, which is still accepted in many quarters as fundamentally historical; though now with wide reservations everywhere. Consequently prehistoric archæology and ethnology have here come into existence as accessory and supplementary studies, and the data of the literary tradition have been used, as was inevitable, as a working hypothesis; which, it is only fair to say, has served its purpose fully as well as there was every reason to expect. Consequently again, any account of the more recent and more

strictly anthropological work in this field must stand, if it is to be intelligible, in close relation with the data and assumptions, which have so mainly determined its course.

ANCIENT TRADITIONS AND MODERN INTERPRETATIONS.

1. The data upon which Greeks of the sixth and early fifth centuries relied for the reconstruction of their own history consisted wholly of traditional anecdotes, appended to traditional genealogies, or grouped, in more or less organic connection, round equally traditional events, such as an invasion of the Troad, or an exploration of the Euxine, or the adventures of a typical navigator like Odysseus. Many of the lays in which these anecdotes were preserved can be traced with some probability to their places of origin, which range from Cyprus to the islands off the west coast of Greece, and from Thessaly and the Troad to Crete. All profess to represent the civilisation of the Ægean area at a period removed by several centuries from the point at which the Hellenic world emerges into history; and the traditional chronology of historical Hellas went up to an era which is slightly later, but approximately contemporary with the latest episodes of the Epic poems. Now though the lays which display the greater literary skill and the maturer idiom give a less vivid and more conventional picture; and though occasional allusions occur to customs and beliefs which are characteristic of Hellenic culture, those others which Greek tradition reckons primary, namely, the Iliad and the Odyssey, are obviously at close quarters with their subject; and if there is one thing certain about the civilisation of the "Homeric Age" thus described, it is that it differs in nearly every important feature from that of the "Hellenic Age" of historical Greece.

2. The Greeks, in fact, themselves regarded their earliest literature as antedating the chronological limits of their history, and already perceived that they belonged to a different order of things. In particular, the ethnography of the Ægean, preserved in an admittedly late and degenerate lay, differs uniformly from that of historic Hellas as far back as it can be traced, and those names are almost

absent by which the Greek race was denoted historically; by its western neighbours as Ἕλληνες, by its eastern neighbours as Ἰάονες (Javan). This inconsistency was attributed by the Greeks themselves to a period of invasion and migration analogous to that which broke up the Græco-Roman civilisation of the Mediterranean. Dorian, Thessalian and Bœotian mountaineers were represented as forcing the barrier, or descending from the highlands, of the Balkans, bringing the old established "Achæan" civilisation to an abrupt close, and reducing the Ægean, and mainland Greece in particular, to a chaotic and barbarous state, the recovery from which is the dawn of the historical Hellenic genius.

3. Some facts within their own experience went to confirm this view. Here and there tribes retained the names and the mode of life of the earlier age; or a noble family professed to trace its descent beyond the limits of current genealogy, and to identify itself with a Royal house of Achæan princes; and here and there ruined fortresses remained, or ancient tombs had been disturbed, which seemed to confirm the description of Achæan splendour in the ballads.

4. Thus much had been established from the beginning of Greek History onwards, and had not been seriously shaken by successive attempts to discredit the traditional view. The theories that the lays are comparatively late compositions, and that they stand in no close relation to a pre-Hellenic age; that the Achæan Age is an invention, and the Period of the Migrations a hypothesis to explain its inconsistency with the facts of historical geography, all prove too much, and may be met with *argumenta ad hominem* from the same traditional data. No literary critic of the Epic has yet purged himself of a sediment of traditional preconception; and, in proportion as one or another has attempted to do so, he has been reduced to a merely agnostic position.

5. Further, until very recent years, every attempt which was made to elucidate the civilisation of the Homeric Age by the monuments of early Greek civilisation rested upon

the assumption that the representations of dress, armour, etc., of the sixth, fifth and fourth centuries B.C., were valid illustrations of poems which at the latest belonged to the seventh, and on an average were assigned to the ninth or tenth century. The reason of this was that Homeric subjects in Greek art are uniformly furnished with accessories of the age of the artist, and that until the study of Classical Antiquities began to be infected with the "evolutionary notions" which had already long been current in all other departments of Ethnography, the attention of students of Greek art and culture was strictly confined to mature and decadent art; everything which could not be assigned to a century subsequent to the fifth was either dismissed as barbaric, or discounted as a "Phœnician importation"; the part which "Phœnician" fables, ancient and modern, have played in the historical study of the Mediterranean area will be considered briefly later on. Such, for example, was the received opinion—so far as there was one—of such discoveries of pre-Hellenic culture as those of M. Fouqué's expedition to the Island of Santorin (Thera, 1862), where, in the course of a geological investigation, a primitive settlement was found under a thick bed of volcanic debris, or of those of MM. Salzmann and Biliotti (1868-71), who in searching for antiquities in Rhodes found at Ialysos, for the British Museum, a magnificent collection of early vases which are now known to be Mykenæan, and second only in quality and variety to those from Mykenæ itself. The Santorin settlement was simply taken to confirm the legend of the Phœnician colony of Kadmos (Hdt. iv., 147), and the vases from Ialysos were explained as the barbarous but immediate predecessors of those from Kamiros, were classed with them as "Græco-Phœnician," and were referred to the seventh century, in spite of the absence of Egyptian objects of the twenty-sixth Dynasty, and the presence of objects of the eighteenth: a view which in certain quarters is not yet quite extinct.

6. It was not till 1871 that Dr. Heinrich Schliemann was enabled to execute his lifelong ambition of testing with the spade the Greek tradition that the site of the Græco-

Roman town of Ilion was also the site of Homer's Troy. The tradition had indeed been sorely handled by Demetrios of Skepsis, a local antiquary of the second century B.C., on the geological ground that the Plain of Troy is of recent alluvial formation; and by other critics on the score of inconsistency with the Homeric narrative. But the Bali Dagh, the site suggested by Demetrios, and in fact the only alternative, is far more inconsistent, and is put absolutely out of question by Dr. Schliemann's discoveries. In successive seasons (1871-3, 1876-82) he laid bare not one, but six cities, built one after another on the same site, and forming an accumulation of walls and debris some thirty feet deep; and, among these, two additional layers have been distinguished in the confirmatory excavations of Dr. Dörpfeld, 1892-94. The latter, however, indicate that Dr. Schliemann's earlier work was not, from the circumstances of the case, sufficiently closely watched throughout, and that in some cases objects were probably picked up at lower levels than those to which they properly belong. In particular, it is not clear that the *cache* of jewellery and plate known as the "Great Treasure of Priam" was *not* hidden originally in a shaft of some depth.

7. Dr. Schliemann claimed as the Homeric Troy the Second Town from the bottom, which had perished by fire, and in which the "Great Treasure" was found. But the Sixth Town, which Dr. Schliemann described as Lydian, was shown by Dr. Dörpfeld in 1892-93 to be larger and more important than was at first supposed, and to correspond closely with the remains found subsequently at Mykenæ and elsewhere.

8. With the same purpose in view of testing the Homeric tradition, Dr. Schliemann proceeded in 1875-6 to excavate the citadel of Mykenæ, in the Peloponnese, the traditional centre of the Achaian feudal confederacy. Here the results were equally unexpected, but no less confirmatory of the legend. A civilisation was brought to light wholly un-Hellenic, but far from barbarous; greatly in advance of all but the latest layers of Hissarlik, and presenting already the marks of decadence after a protracted

career. The pottery, the personal ornaments, and in fact the whole cycle of the art, were at once recognised as identical with those of Ialysos, while the stone-fenced burial-place discovered just within the "Lion Gate" of the citadel, with its six "shaft graves" and their enormous wealth of gold vessels and ornaments, seemed ample confirmation of the legendary wealth of "golden Mykenæ," and was proclaimed, in the first enthusiasm of the discovery, as the tomb of Agamemnon himself. The further researches which have been made almost continuously from 1886 onwards by M. Tsountas for the Greek Archæological Society have confirmed in all essential points the first general impression, but the discovery of later tombs in the lower quarters of the town has made it possible to trace an order of progress and to extend the limits of the period.

9. Subsequent excavations at Tiryns and Orchomenos by Dr. Schliemann, and on a number of other sites in Greece and the Ægean Islands by the Greek Archæological Society and the foreign Institutes in Athens, have demonstrated that this civilisation, which has acquired the provisional name of Mykenæan, is widely represented in the Ægean area and especially in its southern part; that its influence extended over the Central and Eastern Mediterranean from Sicily to Cyprus; that it penetrated, intermittently at all events, into Egypt, where its apparition can be approximately dated, and whence it imported much, and borrowed somewhat, but without losing its own individuality; and, most striking of all, that, after a long period of apparently continuous maturity, it falls into a sudden decadence; leaving, to all appearance, just the same gap between itself and the first traces of Hellenic Art, as we have noted already, on the literary side, between the Homeric Age and the beginning of Hellenic History. It should be further noted, however, that in the last few years many facts have come to light, especially in Attica, in Crete, and, most of all, in Cyprus, which seem to indicate how that gap may eventually be filled. It is from the pottery, almost without exception, that the leading indications have been derived. Fragments of baked clay

are practically indestructible, even though the vessels which they composed have been shattered. Moreover, all the unrefined varieties of clay, and many even of the best levigated, present features by which their place of origin may be recognised. Consequently, in this material, modelling and decoration can be perpetuated as in no other way; and, what is more important, the intrinsic worthlessness of earthenware has often preserved it from the displacement and destruction which almost inevitably overtake objects of gold, bronze, and marble. The resulting preponderance of ceramographic references in the bibliography which follows these notes must therefore be taken as indicating the character of the evidence which is most accessible, and of the method which has actually proved most fruitful: not that the pottery really took so large a place in primitive art as might be inferred from its actual abundance, and its scientific importance.

10. Consequently the study of Early Man in the Ægean has entered within a few years on a new phase, and presents the following problems: (1) To reconstruct in detail the history of the Mykenæan civilisation; its origin, its character, range and influence, and its decline; (2) to investigate the causes of that relapse into barbarism, which both literature and archæology attest; (3) to determine the ethnological position of the race, or races, who originated, maintained, and overthrew it, and their relationship with the historic inhabitants of the same area; and (4) as a special study, to determine the relation in which the Hellenic traditions of the Achæan Age, and the lays in which they were preserved, stand to the civilisation which they certainly seem to commemorate, and which owes its discovery simply to the application to them of a new method of criticism.

## (1) THE FIRST KNOWN CULTURE OF THE EASTERN MEDITERRANEAN.

11. Palæolithic Man seems to have left no traces in the Levant comparable with those in North Europe, or with the plateau and upper-gravel flints of the Nile Valley. But the scarcity of evidence is partly due to the indifference of

the natives to such objects, and to the almost complete diversion of trained research into more obvious and attractive departments; partly also to the comparative rarity, except in Egypt, both of workable flints and of the high-level gravels in which they are usually preserved. From Greece itself only one palæolithic implement is recorded hitherto; a flint celt from Megalopolis in Arkadia (*Rev. Arch.*, xv., 16 ff.).

12. Neolithic Man, however, can be traced over the whole area. Masses of hard crystalline rock are frequent and accessible, and furnished implements of characteristic types; short full-bodied celts, more or less markedly conical behind, and ground to a rather obtuse edge. Obsidian was largely exported from Melos and Thera to the neighbouring islands, and to the mainland of Greece, and was worked up at Korinth and on several sites in Attica. Jade of good quality was sent from Asia Minor outwards across the Ægean; but it is not yet clear whether the source of the common green variety is in Asia Minor itself or further east: the jade implements become commoner eastwards, and the finest collection from any single neighbourhood is that brought by Mr. D. G. Hogarth in 1894 from Aintab in N. Syria (Ashm. Mus., Oxford).

13. Tombs of this stage of culture have not been found—or sought—in sufficient numbers to justify discussion or to contribute any facts of importance. The necropolis of Psemmetisméno in Cyprus, for example, contains besides typical early Bronze Age tombs a still more primitive class, in which the pottery is exceedingly rude, and the characteristic red-polished ware of the early Bronze Age is wanting; but though bronze is absent, no stone implements are present. On the other hand the few tombs recorded as containing stone implements are brought down by their general character well within the Bronze Age.

14. Exception must however be made in favour of the Nile Valley, for Professor Flinders Petrie in 1895 found, at Ballas and Nagada, both tombs and villages of an invading race, apparently Libyan, which had brought the art of flint working to unequalled proficiency, and remained

almost ignorant of the copper which was already in fairly common use under the Sixth Dynasty, which immediately preceded their irruption into Egypt. But the significance of this discovery and of our very limited knowledge of the Libyan people and their civilisation will be better discussed at a later stage.

15. On the other hand, several Settlements of the Neolithic Age have been examined. Typical is the lowest town of Hissarlik, though it has actually yielded a few simple copper weapons. The implements are of local flint and imported obsidian, of green-stone and allied rocks from the interior of the Troad, and of jade; some of the common green Anatolian, others of finer yellowish kinds (*cf.* the specimen in Ashm. Mus. attributed to Melos), and one small celt of the pure white variety which is not known to exist native except in China.

16. The fortifications and house walls of the "First City" are of very rough unhewn rubble; its pottery is of local fabric, made wholly without the use of the potter's wheel, and almost uniformly tinted black by a carbonaceous pigment, intentionally applied and accentuated in the burning; many of the forms are closely allied to those of the neolithic and early bronze ages in Central Europe, and of the corresponding deposits of Greece and Cyprus. This lowest settlement is separated from the rest by a layer of natural soil, which represents an interval during which the site lay desolate; it is therefore distinctly older than the succeeding cities. But the advanced and special technique of the Pottery of the First City, and the fact that, on Schliemann's authority, copper implements already occur, indicate the end rather than the beginning of the Neolithic stage; and the Neolithic evidence from elsewhere is best summarised here, before going further in the series at Hissarlik.

17. Settlements of similar character, but each with its own local peculiarities, occur (1) on an unexcavated site, commanding the Bosphorus as Hissarlik commands the Dardanelles. (2) On the "Kastri" near Achmet-aga in Eubœa, a low hill fortified with earthworks and approached by a hollow way, like the hill camps of the south of England.

(3) Beside Dombrena near Thebes in Central Greece: the site has not been described, but neolithic implements are very frequent: among them is a potter's burnisher of white quartzite (Finlay Coll., 280. Athens). (4) On the Acropolis of Athens many implements and vases were entirely confused by the levelling of the summit in the fifth century B.C.; on the south side (in the space afterwards known as the Πελαργικὸν) is a layer of neolithic pottery with obsidian flakes and a potter's burnisher, almost wholly destroyed by the recent excavations, and only preserved where it is left to support the fragmentary walls of the Mykenæan settlement. The material of the pottery is Ilissos mud, not the Kerameikos clay of the Kephissos valley. (5) Beyond the Ilissos, between Hymettos and the sea, the exact site is unknown, potsherds are common on the surface. The many stone heaps in this district seem to have been accumulated from off the fields on to barren spots; two, opened south-east of Kara in 1895, were quite barren; a tumulus north-east of Kara, surreptitiously opened, contained a Mykenæan interment (Ashm. Mus.). (6) Primitive pottery is common on the west end of the cliff which runs along the coast from New Corinth nearly to the site of Lechaion.

18. The "Second City" of Hissarlik has marked points of similarity with the first, but represents a decided advance, and has notable characteristics of its own. The walls, great and small, are of better masonry below, and of sun-dried brick above, with bonding courses and terminal uprights (antæ) of timber; the centre of the fortress is occupied by a "chief's house," consisting of three oblong buildings with portico entrances at one end in a courtyard entered by a covered gateway. The pottery is still of unlevigated clay, and mostly hand-made; it is no longer blackened as before, but either left as it is, or covered with a red slip, which continues to occur in the layers above; new and characteristic forms appear, some peculiar, others again common to Central Europe, to the Greek islands or to Cyprus. Stone implements are still in common use, but copper and bronze begin to be frequent though they are still of simple

types. But the pre-eminent feature of the Second Town is the discovery of more than one buried "Treasure" of gold and silver jewellery and vessels, the latter certainly of local manufacture, for the forms closely correspond with characteristic types of the pottery.

19. The Second Town perished in a general conflagration, and the Third, Fourth and Fifth Towns above it never attained to anything like its magnificence. They mark, however, a gradual advance of civilisation and form a transition, more and more rapid as it proceeds, towards the Sixth Town, a quite distinct and well-marked settlement of "Mykenæan" invaders, in which imported pottery, and native imitations of this, occur alongside of fully developed indigenous forms, which again recall in characteristic details many Central European types. This Sixth Town is the only one which can be even approximately dated chronologically; it is certainly prior to 1000 B.C., and need not be later than 1300; the Fifth and lower settlements must of course necessarily be older than this.

20. It has been already hinted that the "Treasure of Priam" *may* belong to a period somewhat later than the Second Town, though not so late as the sixth or "Mykenæan" Town. Whether this be so or not, we have in the jewellery an early example, perhaps a prototype, of the characteristic gold work of the Mykenæan Age; but if the "Treasure" is contemporary with the layer in which it was found, the time limit for the whole series at Hissarlik must probably be contracted downwards. In any case we must believe that the earliest civilisation of Hissarlik was not so wholly barbarous as appears at first sight.

21. Imported objects found at Hissarlik indicate a wide range of foreign connections. The fragments of porcelain point to Egypt; the lapis lazuli axe from a neighbouring site, to Turkestan; the silver vases probably to the eastern half of Asia Minor; the types of the bronze implements alike to Cyprus and to the Danube Valley; and the amber to the shores of the Baltic. This wide commerce does not, of course, imply direct intercourse, but, from its geographical

position on the Hellespont, Hissarlik must have been a point of convergence for any trade between the East and Europe, and the catalogue of the allies of the Trojans in Iliad II., though it refers to a later period, ranges them (1) up the Hebros Valley into the Balkans, and along (2) the North and (3) the West coast of Asia Minor; *i.e.*, along three well-known routes of early trade.

22. The metallic objects of Hissarlik are of particular value as links between two principal copper-working areas, Cyprus and Central Europe. The latter really falls beyond our present view, but must be noted—mainly to be rejected—as a possible source of the early Mediterranean Bronze.

23. The use of copper in Cyprus goes back far beyond the point where it can be dated with any certainty, and everything goes to show that, while southwards, namely, in Egypt under the Fourth Dynasty, Cypriote types appear from the first side by side with others which are probably Sinaitic, northward the same types extend, past Hissarlik, into the Danube Valley, and are imitated and amplified into derivative forms throughout Central Europe; returning, almost unrecognisable, into the Mediterranean area in the series from Spain, which is clearly not directly derivative, and may be of comparatively late origin.

24. The obvious suggestion that Central Europe may have worked copper independently is met (1) by the comparison of the secondary forms,—*e.g.*, only in Cyprus can the actual synthesis of double-bladed axe heads, by welding two simple ones, be observed; (2) by the fact that, along with the characteristic and indigenous metallurgy, the ceramic technique of Cyprus, with red hand-polished surface and incised ornament filled with white earth, can be traced across Asia Minor and into South-eastern Europe; the red slip as far as Brus in Transylvania; the ornament into the Mondsee of Lower Austria, and the pile-dwellings of Switzerland, becoming ever more mongrel and degenerate as it proceeds.

25. It is important to note that at Hissarlik a return current is already evident; the pottery and the metal im-

plements reproduce European types as well as Cypriote, and this is confirmed, not only by traditional and ethnological considerations, but also by the occurrence, somewhat later, in the Ægean area, not only of frequent amber, but of characteristically Danubian types of bronze implements.

26. The Bronze Age civilisation of Cyprus is, thanks to repeated researches, far more continuously and completely known than any other part of the area. It was undoubtedly of very long duration, and certainly follows that of the Stone Age without change or break; and it is no exaggeration to say that, until a period between the twelfth and the eighteenth Egyptian Dynasty, Cyprus was in all essential respects in advance, not only of the coasts of Asia Minor and the Ægean, but even of the coast of Syria and Palestine.

27. All the earliest weapons, whether in Cyprus or elsewhere, in Egypt, or the Levant, are of almost pure copper. Tempering is effected, not by alloying with zinc or tin, or, as in the Caucasus, with antimony from the natural double-sulphide ore, but by "under-poling" the copper so as to leave it hard and even brittle from the presence of copper oxide. The same applies to the Egyptian copper weapons of the fourth, fifth, and even sixth Dynasty; but Egypt, though later on it has important connections with Cyprus, obtained its first copper from the mines of Sinai, and has a set of typical forms peculiar to itself. Cyprus, however, supplied the Syrian coast with copper weapons down at all events to the time of the eighteenth Dynasty. Stone implements are very rarely found in Cyprus, and it is possible that either the island was not reached much before the beginning of the Bronze Age, or that its wealth of copper was discovered at once, and superseded the stone age prematurely. In its earlier stages metallic implements are rare, and the pottery—always made by hand—is covered with a bright red glaze which was polished with a stone or bone rubber (horse teeth were commonly used), and ornamented, if at all, either by incised lines or by pellets of clay rudely modelled after plants, snakes and

horned animals. In its earlier part, therefore, the civilisation, so far as it is known, is peculiarly uniform in character, and displays no trace of foreign influence; except only that the characteristic red-polished glaze of the pottery, already mentioned, is almost identical with that of the Neolithic Libyan people of Ballas-Nagada, and of their "Amorite" kinsfolk in South Palestine. Even here, however, there is no evidence at present of imitation on either side. The strong influence which Cyprus exercised, through its copper trade, over the neighbouring coastland is best illustrated by the discoveries of Dr. Bliss at Tell-el-Hesy, on the coast plain of Palestine (Philistia), some sixteen miles from Gaza. The site consists of an acropolis with eight "Cities" superimposed as at Hissarlik. The mass of the remains represent an indigenous "Amorite" civilisation of low type, related, according to Professor Flinders Petrie, to that of the Libyan invaders of Ballas-Nagada. But bronze appears from the bottom of the series upwards, and iron already in City Four, which with City Three appears to be contemporary with the eighteenth Dynasty and the Mykenæan Age. The bronze types are derivative, partly from Cyprus, partly from Egypt; and Cypriote importations of the later painted fabrics occur in Cities Two and Three together with native imitations. The red-polished pot fabric of Tell-el-Hesy, however, belongs to the Amorite civilisation, and is not necessarily borrowed from that of Cyprus.

28. In the latter half of the Bronze Age, Cyprus with characteristic conservatism fell for a while slightly behind its neighbours, and began to import ornaments and articles of luxury from Egypt and the Syrian and Cilician coasts. In this stage the red-polished ware tends to deteriorate in colour and finish; the bronze weapons become more numerous, and contain a higher percentage of tin, and occasionally jewellery of coarse silver-lead, all of native make, is found in the more richly furnished tombs. Babylonian cylinders occur rarely as imports, with a multitude of characteristic native cylinders. Egyptian scarabs and porcelain beads are also found rarely; and with these again a very common variety of coarse crumbly porcelain badly glazed

with a very faint blue: the pigment was evidently difficult to obtain, and was used but sparingly by the native artist. But meanwhile the discovery of the art of ornamenting the natural surface of clay vessels with an encaustic umber pigment, wherever it may have originated, seems to appear in Cyprus (where umber is extensively worked) at least as early as anywhere else; first in company with, but later almost wholly superseding, the older mode of incising linear ornaments on a prepared and polished surface.

29. The simply painted pottery is followed, though not immediately, by several other fabrics which, though probably native to Cyprus, are represented in some quantity on Egyptian sites of the twelfth Dynasty and later dates, and also in equivalent layers in the stratified mound of Tell-el-Hesy, in the "Hittite" Sinjirli, and sporadically elsewhere; one very characteristic variety, with dark body, white chalky slip, and black almost glossy paint, has been found even so far afield as the Island of Thera, the Acropolis of Athens, and the "Sixth City" of Hissarlik.

30. The specimen from Thera was found in company with vases of a distinct and local style; some still with coloured surface and incised ornament, others with simple painted patterns. The forms, however, and the whole fabric, are quite distinct from those of Cyprus, and show a graceful freedom which is quite new; though they are clearly derivative from a ceramic of the Hissarlik type. Most important of all, the wholly geometrical and mainly linear ornament which has been hitherto universal is combined with or replaced by a thoroughly and vigorously naturalistic study of animal and vegetable forms, and, in combination with the latter, spiral motives appear, hitherto unknown but destined to a long and eventful career. These naturalistic and curvilinear designs are not only represented on the pottery, but are also frescoed upon the plastered walls of the houses; they may consequently be taken to be locally characteristic. The settlement at Thera was found beneath a thick bed of volcanic debris, and had evidently been suddenly abandoned; metallic objects are rare, but this may well be due, as M. Tsountas suggests, to the flight of the inhabitants—for no

skeletons were found; and a few copper implements and gold ornaments remained to confirm the inference from the pottery as to its position in the series.

31. Settlements and tombs of the same character have since been noted in many islands of the Archipelago, especially in Syros, Melos, Antiparos and Amorgos; and this "Cycladic" type of ornament and general civilisation is not only closely paralleled by the earliest remains at Mykenæ, Tiryns, Athens and elsewhere, but is connected by an almost continuous series with the fully developed art and civilisation of the Mykenæan Age itself.

32. It should be noted that though Cyprus appears to have exported its own manufactures to the Ægean during this period, it was not in a position to influence or direct the Cycladic culture. But still less is there any trace that the younger and more vivacious school reacted at all upon the elder; this was reserved for the full-grown culture of Mykenæ.

33. It is at this period that the Cretan evidence, though as yet miserably incomplete, becomes of crucial importance. Crete shares, to begin with, the early bronze age civilisation of Hissarlik and Cyprus, resembling the latter more closely; but it is not till the Cycladic stage is reached that we have more than the most fragmentary evidence. In the Cycladic period and in the succeeding age Crete was almost literally ἑκατόμπολις, the "island of an hundred cities," and certainly exercised a vigorous and continuous, perhaps even a predominant influence upon Ægean civilisation. At this point the wealth and variety of Cretan decorative art become conspicuous, and a chronological point of the very first importance and a clue to the origin of some characteristic motives are given by the recent demonstration of a frequent and fertile intercourse with Egypt in the time of the twelfth Dynasty. On the one hand, a very peculiar and local fabric of pottery from Kamárais in Crete has been found in twelfth Dynasty layers at Kahun; on the other, the Cretan types of bronze implements are typically Egyptian, and twelfth Dynasty scarabs were not only frequently imported, but commonly imitated. In fact it is very probably from this

quarter that the spiral motives, which are dominant in the Egyptian Art of the twelfth Dynasty, were introduced into the decorative repertory of Ægean art.

34. The seal-stones engraved with Egyptian and derivative spirals are closely associated in Crete with others bearing groups of symbols, more than eighty of which have been recorded, and shown to be hieroglyphic, by Mr. A. J. Evans. They exist in two series, of which the earlier is fully pictorial and naturalistic, the later conventionally abbreviated into linear forms. Some of the former are closely analogous to certain Egyptian, others to certain "Hittite" hieroglyphs from Kappadokian monuments; many of the latter are identical with *graffiti* on twelfth-eighteenth Dynasty pottery from Kahun, Tell-el-Hesy and elsewhere, and some are probably prototypes of symbols which persisted in the Phœnician, Greek and Lykian alphabets, and in the Cypriote syllabary. This hieroglyphic system is not confined to Crete, though it is far best represented there as yet; the pictorial seal-stones are distributed over the Cycladic area; and two inscriptions in the linear character have been found on vases at Mykenæ. Dr. Kluge, of Magdeburg, believes that he can translate these hieroglyphic inscriptions into a dialect of Greek.

35. We now come to what is, even literally, the Golden Age of the early Mediterranean cycle. "Mykenæan" Art is still best and most completely illustrated by the long series of discoveries in the plain of Argos, which at once revealed its existence, and have given to it a name. The monuments and the civilisation of Mykenæ and Tiryns have been repeatedly, though never yet really adequately, described, and have given rise to the most divergent theories as to their date, their origin, and their relations with what precedes and follows them. The following points are those which are chiefly made clear by the most recent researches.

36. The limits within which Mykenæan sites are distributed may now be defined with some approach to accuracy, and no less the wider area over which Mykenæan civilisation had a living influence. With the exception of

the "Sixth City" of Hissarlik no Mykenæan settlement is known on the mainland of Asia Minor. Isolated vases are reported from Pitane in Æolis, from Mylasa in Karia, and from Telmessos in Lykia, and the early necropolis of Termera (Assarlik) near Halikarnassos (Budrum), though of distinctly indigenous character, is strongly influenced, at the very end of the period, by late Mykenæan models from the neighbouring islands. Among the latter, besides the great settlement at Ialysos in Rhodes, every island appears to be represented from Rhodes southwards to Crete, and northwards as far as Patmos. Both in Melos and in Thera Mykenæan settlements are found distinctly superimposed on the Cycladic already mentioned, and others are indicated by isolated finds throughout the Archipelago. On the mainland of Greece, Lakonia is represented by two sites Kampos and Vaphio (Amyhlæ), the latter with a princely "beehive tomb" like those of Mykenæ; Argolis by Mykenæ, the Heraion temple-site, Tiryns, Nauplia, Trœzen, Epidauros, and the islands Kalauria and Ægina; Attica by Athens, Eleusis, Acharnæ (Menidi), Aliki, Kara, Spata, and Thorikos; the rest of Central Greece by Megara, Antikyra, Thebes, Tanagra, Levadia, Orchomenos and several smaller sites in the Kopais marshes; North Greece by Pagasæ (Dimini near Volo) in Thessaly.

37. In the West there are no Mykenæan settlements known further than Kephallenia and Ithaka; but Mykenæan vases occur in domed rock tombs at Syracuse, and there is much indirect evidence of Mykenæan influence on the later Bronze Age style in Sicily and South Italy. Further than this, it is clear that on the Adriatic coast of Italy Mykenæan imports and models determined the character of the later Bronze Age, and that in the transition from Bronze to Iron at Hallstatt in the Tyrol, a definitely Mykenæan strain can be detected. But in both these cases the contact is with later and already quite decadent types, such as are represented in the Lower Town of Mykenæ; in particular fibulæ are always present, and of these the secondary and distinctly Sub-Mykenæan types are only very rarely absent.

38. Eastwards, Mykenæan imports are found frequently

in Cyprus, in the latest class of Bronze Age tombs, and give a very distinct character to the necropoleis of Episkopi (Kurion), Enkomi (Salamis), Pyla, Nikolidhes, and Laksha-tu-Riu. Native imitations increase in frequency, and eventually supersede the importations and fix the leading features of the art of the early Iron Age, *e.g.*, at Kuklia (Paphos), Lapathos and Katydata-Linu. In Egypt again, Mykenæan importations are found in great quantity, associated with the later Cypriote fabrics and stimulating copious native imitation in layers of the eighteenth Dynasty at Illahun, Gurob, Tell-el-Amarna. These last finds confirm the date already inferred from the occurrence of eighteenth Dynasty scarabs and porcelain ornaments at Ialysos and at Mykenæ, and fix the general chronology of the Mykenæan Age beyond all question. The contrary opinion, that the Mykenæan civilisation immediately precedes the Orientalising culture of the seventh-sixth centuries, and consequently itself descends as late as the eighth-seventh centuries, has been vigorously urged by a few English students, but has long been abandoned by all who have had first-hand experience of the conditions of discovery. The premature contention that the fortress of Tiryns was Byzantine deserves mention, but is obsolete.

39. It is in Egypt also, moreover, that the first notice occurs of the actual peoples who transmitted the civilisation in question, and this in a peculiarly suggestive connection. In the fifth year of Merenptah (1225) and under Rameses III. (1180-1150) the western frontier of Egypt was seriously threatened by a Mediterranean coalition, of which the Libyans were the principal members, but which included under the general description of "the peoples of the isles of the sea" a number of tribes whose names, though much distorted in the Egyptian hieroglyphic records, strongly resemble those of Achaians, Danaans, Ionians, Teucrians, Tuscans or Tyrrhenians, and perhaps Sicilians and Sardinians. Neither these names, of course, nor yet the apparent resemblance of their arms and furniture, as depicted in Egyptian paintings, can give more than a plausible presumption of identity either with historical Ægean races or

with the representatives of Mykenæan civilisation. But the analogies are on all sides so close, that the identification is usually accepted, and that as soon as even the outlines of the history and civilisation of Libya during the Bronze Age are ascertained, we shall be in a position to formulate the real relations which then existed between Libya and the Ægean, and probably also to trace more clearly to its source the very remarkable realistic instinct which distinguishes the art of the Ægean from all contemporary styles.

40. The sudden collapse of the Mykenæan civilisation, which was indicated to begin with, is roughly coincident with the first appearance of Iron in common use in the Levant, and the attempt has been made, though on no direct evidence, to connect the two tendencies. All the facts go to indicate that, so far as the Mediterranean area is concerned at all events, iron makes its appearance first on the Syrian coast, in the period which immediately succeeds the downfall of Egyptian suzerainty in that area under the nineteenth and twentieth Dynasties: *e.g.*, at Tell-el-Hesy iron occurs down to the fourth "City" (= eighteenth Dynasty). The ambiguity of the Egyptian allusions under the eighteenth and previous Dynasties makes any earlier date uncertain, and iron has not been actually *found* in Egypt before the twenty-sixth Dynasty, 650 B.C. In Cyprus, where the evidence is completest, and where abundant native ores have certainly been worked from an early period, iron suddenly becomes very common just at the point when Mykenæan vases are ceasing to be imported, but when, on the other hand, Mykenæan conventions have already begun to influence profoundly the native scheme of ornament. At Mykenæ itself iron occurs first as a "precious metal" and in the form of signet rings, at the stage where decadence begins to be rapid, but it is not put to practical uses till the moment where the series breaks off, and the same is the case in other Mykenæan sites in the Ægean; one iron sword was found in the Vaphio "beehive".

41. Up the Adriatic again it is with the early fibulæ and quite degenerate Mykenæan art, that iron makes its appear-

ance, at Novilara; and at Hallstadt; and here again, both in tradition and among the finds, there is evidence that the metal became established first as an ornamental rarity, and only subsequently as a substitute for bronze.

42. But though in its principal centres Mykenæan civilisation has all the appearance of having been suddenly and violently extinguished, this must not be taken to be universally the case. In Argolis (at Tiryns, and the Heraion), in Attica, and in Melos, for example, there is every reason to believe that the Mykenæan civilisation survives, though in very degenerate phases, into the period when Iron and the characteristic art of the early Iron Age are already well established; and at Nauplia and the Attic Salamis, and still more in Crete, in Karia, and in Cyprus, the stages may be clearly traced by which, so far as in it lay, the Iron Age took up its inheritance from the Age of Bronze. The nature and the result of this transference are easily summarised.

43. It has been already indicated, firstly, that throughout the Eastern Mediterranean, in fact throughout the whole range of the Mediterranean Early Bronze Culture, the indigenous system of decoration is instinctively rectilinear and geometrical; secondly, that in the Cycladic area and in the Middle Bronze Age a quite irreconcilable and purely naturalistic and quite heterogeneous impulse appears; and thirdly, that the fully formed Mykenæan style, when it appears, is, in spite of its far superior technical skill and elegance, already beginning to stagnate in many departments; the gem-engraving and modelling developing last, and retaining their vigour and elasticity latest; whereas the ceramic decoration, which appears in its noblest form at Thera and at Kamárais, is the first to exhibit the conventional and mechanical repetition of a shrinking assortment of motives. We may now add, fourthly, that this failure of originality permitted of a recrudescence of the rectilinear instinct which, though overwhelmed for the time by the naturalistic and curvilinear principles, had co-existed with them throughout; and that both floral and spiral motives, once allowed to repeat themselves without

reference to their models, are transformed automatically into the latticed triangles and mæanders, which are the commonplaces of rectilinear design.

44. At this point the survey must close, for now, on geometrically engraved tripods, and geometrically painted vases, appear Hellenic inscriptions in alphabetic characters. Borrowed Oriental, and especially Assyrianising, motives intrude themselves into the panels of the rectilinear ornament, and attempts are made, however ineffectual, to represent first animal and then human forms. Now, in the development upward out of the "Dark Age," Hellenic history begins to reckon onward from the Trojan Era and from Olympic and kindred lists; and Hellenic art no longer forward from the eighteenth, but backward from the twenty-sixth Dynasty.

LEVANTINE ETHNOLOGY, AND SUMMARY (*to follow*).

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# THE GRAPTOLITES.

THERE is, perhaps, no better illustration in geology of the value of detailed work than that which is furnished by the group of organisms, to the consideration of which this article is devoted. Formerly viewed with suspicion by biologist and geologist alike, and frequently altogether ignored, we find the graptolites now treated with respect even by those who have not devoted special attention to them. Their value is generally recognised as aids in the determination of the age of strata, but besides this, a detailed study of the group will undoubtedly throw light upon the physical and climatic conditions under which the strata containing graptolite remains were deposited, and also upon the evolution of the various forms of graptolites. Every one will admit that the appreciation in which graptolites are now held is largely due to three papers by Professor Lapworth, one of which treats of these organisms from a biological (1), and the second (2) and third (3) from a stratigraphical point of view; and the publication of these papers is doubtless largely responsible for the appearance of a large number of memoirs devoted to a study of the group under consideration which have been written of recent years. These recent memoirs it is the object of this paper to consider.

The memoirs, early and more recent, treating of the graptolites are scattered through a variety of publications, but an excellent bibliography compiled by Otto Herrmann and published in his *Inaugural Dissertation* (4) gives a list of these memoirs up to and including the year 1883. Even with this guide the student has much difficulty in obtaining access to some of the publications, and a general monograph of the graptolites has yet to be written. In the list of "Monographs which are promised or are in course of publication" appended to the last "Monograph of the Palæontographical Society" we note "The Graptolites," by Professor Lapworth, and all geologists must hope that ere

long the professor will give to the world the full results of his prolonged researches into the history of the group. This monograph must necessarily be confined to an account of the British graptolites, but when that is complete surely Professor Lapworth will treat of those of other countries also.

The graptolites, at one time referred by some writers to the Hydrozoa, by others to the Polyzoa, are now generally admitted to belong to the former class, though the exact value of the sub-division is not definitely settled, for whereas we find Professor von Zittel in his *Palæontology* treating of them as a sub-order, Graptolithidæ (= Rhabdophora, Allman), divided into the groups Graptolitoidea Lapw. and Retioloidea Lapw., Nicholson and Lydekker (*Manual of Palæontology*) place them in a sub-class (Graptolitoidea). In these works the general structure of the graptolites is described, though, as will be seen in the sequel, one structure supposed to be absolutely characteristic of all graptolites, namely the virgula, is not really so. Comparatively little has been added to the knowledge of the histology of the graptolitoidea furnished by H. Richter (5), though some of his results have been confirmed by Professor Sollas (6); and additional information has been supplied by Professor S. L. Törnquist (7) and Dr. Perner (8). Some of the most important papers published of recent years treat especially of the mode of growth of the proximal portions of the graptolites. The first of these by Törnquist (9) is occupied with a description of sections through several deprionidian graptolites. The author distinguishes the obverse from the reverse aspect of the polypary, and also introduces two terms to distinguish its right and left portions—the "primordial" portion, containing the "primordial" series of hydrothecæ, is marked by the possession of the earliest hydrotheca, whilst the other portion is termed the "second" portion and possesses the second series of hydrothecæ. When the obverse aspect of the polypary is turned towards the observer the primordial series of hydrothecæ is invariably on the left hand. The sicula sends out what the author terms a "connecting canal" which opens into a

"biserial chamber," thus producing a connection between the various parts of the polypary. These features are common to all the forms described by the author, but the forms differ in other respects. In *Climacograptus scalaris* Linn. and *Climacograptus internexus* Törnq. the biserial chamber communicates with two uniserial canals separated from one another by a median septum. In *Diptograptus palmeus* Barr. the septum scarcely extends through half the thickness of the polypary, whilst in *Cephalograptus cometa* Gein. it is "reduced to a narrow fold of the obverse periderm," and in *Diptograptus bellulus* Törnq. it is altogether absent.

Two papers by Wiman (10) treat of the structure of the *Diptograptidæ* and of *Monograptus.* Notices of these papers by E. M. R. Wood and G. L. Elles appear in the *Geological Magazine* for 1895, p. 431. The accounts of the structure of the sicula, and of those parts of the polypary immediately in contact with it, are largely confirmed by Holm in a paper to be noticed immediately, but the statement that the *Diptograptidæ* are monoprionidian because the sicula gives rise to only one bud (which is on the right hand side) involves a special use of the term monoprionidian which will hardly meet with general acceptance.

A most important paper by Gerhard Holm must now be noticed (11). Holm has had the advantage of studying some beautiful material derived from the *Vaginatus*-limestone (of Areing age) from various localities in the northern part of the Island of Öland; the graptolites of this limestone he has succeeded in freeing from the matrix, thus rendering them serviceable for detailed study. (The method of removing the matrix is described by Holm in an article in *Bihang K. Vetensk. Akad. Handl.*, Bd. xvi., 1890.) In the present paper he gives reasons for supposing "that the earlier development of the proximal part—the first three thecæ—in all the bilateral or diprionidian forms of graptolites is in the main the same, and has taken place through the formation of only one bud on one side of the sicula—or first theca, as I believe it is—which side is always the same in relation to the later development of the polypary. From

this bud thereafter is developed partly the second theca, partly the canal—'connecting canal'—which connects both halves of the polypary, and which in the first place gives origin to the third theca (= first theca on opposite side of sicula), and partly also the common canal which connects the second theca with the succeeding ones." He describes the "sicula" which consists of two distinct portions, the "initial part" which he believes to correspond with the original "chitinous covering of the free zooid germ or embryo," and the apertural part which has the same function as a theca and may therefore be justly considered as the first theca. Accordingly Holm's second theca corresponds to Törnquist's primordial one, and his third to Törnquist's second.

The sicula in the bilateral graptolites does not occupy a central position, being partly embraced on one side by the connecting canal, whilst on the other side it is more or less superficial. The sicula side is termed the "anterior," and the other the "posterior". These are used in the same sense as that in which Törnquist employs the terms "obverse aspect" and "reverse aspect". The author gives a full account of the connection between the sicula, the first theca, the first bud, from which "arises almost simultaneously with the left theca the common canal for the left half of the polypary, and the connecting canal which crosses the dorsal side of the sicula and gives origin to the third (or, better, the right) theca lying on the right side of the polypary, and also the common canal for the right side of the polypary," and describes the growth of these in *Didymograptus minutus* Törnq., *D. gracilis* Törnq. mut., *D. gibberulus* Nich., *Tetrgraptus Bigsbyi* Hall, and *Phyllograptus angustifolius* Hall.

He maintains that a virgula cannot occur in any graptolites of the families *Dichograptidæ*, *Dictyograptidæ*, and *Nemagraptidæ*, or in the genus *Dicellograptus* of the family *Dicranograptidæ*. The true virgula commences near the apex of the sicula as a prolongation of the same, and corresponds with the thread-like prolongation of the sicula which has long been known in *Didymograptus*

*gibberulus*, and certainly occurs in many other forms of *Dichograptidæ*. Another filiform appendage which might be spoken of as the false virgula "originates as a result of growth within the apertural end of the sicula at some distance from the initial portion. This later structure stands evidently in no relation whatever to the real virgula, but may be regarded as an apertural spine." The significance of these filiform processes has not yet been fully explained, but the possession of a true virgula must in future be omitted from diagnoses of the characters of the sub-class or sub-order of the graptolites. Holm's researches fully confirm Tullberg's inference that *Phyllograptus* belongs to the family *Dichograptidæ*, and the family *Phyllograptidæ* must now be abandoned. Another interesting point bearing upon classification is the position from which the bud grows out of the sicula. "In *Phyllograptus* it is situated quite close to the apex of the sicula, in *Tetragraptus Bigsbyi* Hall probably slightly lower down, in *Didymograptus minutus* Törnq. somewhat below the middle of the sicula, in *Didymograptus gracilis* Törnq. Mut. still nearer the aperture; but in *Didymograptus gibberulus* Nich. the position is almost the same as in *Phyllograptus*." The reference of the genus *Azygograptus* to the *Nemagraptidæ* on account of the stipe being developed from the central part of the sicula on one side is therefore unnecessary, and the general characters of *Azygograptus* leave no doubt that it belongs to the *Dichograptidæ*; indeed Holm in the paper under consideration describes a form which is possibly intermediate between *Didymograptus* and *Azygograptus*.

The association of a number of graptolites of the same species in a fairly symmetrical manner has long been known. James Hall in plate xiv. of his classic work on graptolites (12) figures a diprionidian graptolite under the name of *Retiograptus tentaculatus*, and in figure 9 is "an illustration of a compound form of the genus," possessing nearly twenty diprionidian stipes diverging from a common centre. James Dairon (13) also figures specimens of *Monograptus* occurring in partly symmetrical tufts, and remarks: "I am now thoroughly convinced that many, if

not all, of the specimens of *Monograptus* may have been fixed to the sea-bottom, or to objects lying or growing on it, and not have been free-floating organisms, as has hitherto been supposed, until at last they were separated from their points of attachment by breakage or some other natural cause". Recently a remarkable description has appeared (14) giving an account of specimens of *Diptograptus pristis* Hall and *D. pristiniformis* Hall from the Utica Slates. In these specimens the stipes occur in "compound colonial stocks which appear in the fossil state in stellate groups". From observations on the specimens, the author infers "that the colonial stock was carried by a large air-bladder, to the underside of which was attached the funicle. The latter was enclosed in the central disc, and this was surrounded by a verticil of vesicles, the gonangia, which produced the siculæ. Below the verticil of gonangia and suspended from the funicle was the tuft of stipes," the latter being so arranged that the "sicula-bearing end of the single stipes appears in the compound colonial stock as the distal one". The paper is only an abstract of one which is promised shortly, and geologists will await with interest a full account of these remarkable specimens. The structure described as a funicle can hardly be looked upon as the analogue of the "organ" described by Hall under that name (which by the way has been proved by Brögger and Holm to be celluliferous in many species, so that Holm is doubtless correct when he says that a funicle has not been found in any graptolite). It is remarkable that the author should explain what he means by the assertion that the chitinous capsule which encloses the "funicle" on the specimens described is identical with the "central disc" of the compound fronds of numerous *Monograptidæ*, for no geologist, as far as I am aware, has described *Monograptidæ* with compound fronds, unless Dairon's specimens be taken as such.

The early writers on graptolites looked upon the number of stipes possessed by graptolites as a character of prime importance in defining genera, such forms as *Dichograptus*, *Tetragraptus*, *Didymograptus* and *Monograptus*

being largely characterised by the possession of eight, four, two stipes and one stipe respectively. In a recent paper by Professor Nicholson and the present writer (15) we have endeavoured to show that this is not the case, but that the character of the hydrothecæ and to a less degree the amount of angle of divergence of the stipes are of importance. We endeavour to prove that certain graptolites underwent development along parallel lines, passing through many-branched, eight-branched, four-branched, two-branched and one-branched forms, thus illustrating the principle of heterogenetic homœomorphy advocated by Mojsisovics, S. S. Buckman and others. If this be allowed, many of the present genera will have to be abolished and new ones formed; but the writers earnestly advocate the retention of the present genera under existing circumstances, and hope that the formation of fresh genera will be deferred until our views are more fully developed or perchance disproved, though we do not think that the latter event is likely.

It will be noticed that the above researches into the morphology of the graptolites deal mainly with the celluliferous portions of the polyparies, whilst the study of the various bodies referred to as concerned in reproduction has not been largely pursued of recent years.

Passing now to the memoirs treating of the graptolites as indices of age of the rocks which contain them, it may be remarked at the outset that recent work has fully established the correctness of the views advanced by Lapworth in his papers on the Moffat series and on the geological distribution of the Rhabdophora. Perner alone has stood out for the anomalous occurrences described by the eminent Barrande in the Bohemian basin, but he does not yet appear to have studied completely the zonal distribution of these organisms in that region, though he has added largely to the number of species occurring in the Lower Palæzoic rocks of Bohemia. The new species described here and elsewhere of recent years it is not contemplated to notice in this article, though they will doubtless give us much information in addition to that we have already obtained

concerning the morphology and phylogeny of the graptolitoidea. It would serve no useful purpose to give details of the numerous papers which confirm the value of the graptolites for purposes of correlation of the strata. In Britain, Lapworth himself has described a number of graptolitic bands interstratified with deposits containing the remains of other organisms in Ayrshire (16). Much remains to be done in this respect, for in order to utilise to the utmost the value of these organisms as stratigraphical indices, it will be necessary to have a complete correlation of graptolitiferous strata of all ages, with those which contain these organisms rarely or not at all. For this purpose all graptolites should be carefully collected and preserved from out of those deposits in which they are not frequent, and are associated with other organisms. They should be looked for especially in calcareous deposits, for as we have already seen, such specimens are particularly valuable as furnishing information concerning the morphology of these fossils. The southern uplands of Scotland have recently been re-examined by the geological surveyors, and it is scarcely necessary to state that they have fully confirmed Professor Lapworth's classification of the Lower Palæozoic Rocks of this region. In England Professor Nicholson and the present writer have defined graptolitic zones in the Skiddaw Slates, Llandovery, Tarannon, Wenlock and Lower Ludlow Beds (17). Messrs. Lake and Groom have detected the *Monograptus gregarius* zone of the Birkhill shales and zones of *Monograptus personatus*, *M. Flemingii*, *M. colonius* and *M. leint wardinensis* near Corwen and Llangollen (18), whilst in a paper which has hitherto only appeared in abstract, Miss Wood and Miss Elles have detected several zones of the Birkhill-Gala beds near Conway. On the Welsh borderland W. W. Watts has found one graptolitic zone of Wenlock and two of Lower Ludlow age on the Long Mountain (19). In addition to this, various other graptolitic zones have been detected in different parts of Great Britain, and the zones of the Moffat area have been traced into Ireland. On the European continent, Linnarsson, Brögger, Törnquist, Tullberg and others have detected numerous graptolite

zones in Scandinavia, a full account of which appears in Tullberg's paper on the graptolites of Scania (20), one of the most valuable of recent contributions to the literature of the graptolites. Törnquist, Perner, Barrois and others have also identified various graptolitic zones in Thuringia, Bohemia and France. In North America the principal contribution is by our own countryman, Lapworth, who has identified a number of graptolite zones in Canada, which are identical with those detected in Europe (21). In Australia T. S. Hall is studying the well-known Areing graptolite fauna, and finds that the graptolites here also are limited to special zones (22). A number of other papers might be quoted to show the general recognition of the utility of graptolites for purposes of correlation of strata, but enough has been said to indicate the manner in which the work is progressing, and the vast amount which yet remains to be done in this connection. I cannot leave this part of the subject without uttering a warning note. More harm is done by a wrong determination than good by a correct one. The graptolites are by no means easy of identification by those who have not made them a special study, and it is particularly desirable that no determination should be recorded by tyros, unless it is absolutely certain, for when once a wrong name has crept into a list it is exceedingly difficult to remove it. I could give several instances of very serious mistakes of this kind which have been made, each of which will have to be corrected elsewhere, but it would be invidious to give names in a general article of this character.

We may now pass on to consider the physical conditions under which the graptolite-bearing strata were deposited. There is very little doubt that they were formed in water of very different degrees of depth, for graptolites are found in arenaceous, argillaceous and calcareous strata. They have mainly been collected from deposits which there is every reason to suppose were formed in deep seas, because a much greater number of individuals occur in a given space under such conditions than when the deposits were formed rapidly. The writer has elsewhere given cases of graptolitic deposits

a few feet in thickness, being represented by thousands of feet in adjoining regions, and one naturally discovers forms more easily in a few feet of strata than in several thousand feet where the process of search rather closely approximates to that for the proverbial needle in the hay-stack. The evidence which is being gathered shows more strongly than ever that the thin graptolite-bearing shales, which for the above reasons have come to be looked upon as the deposits for graptolites *par excellence*, were deposited slowly in waters some distance from continents, and probably of considerable depth. The evidence for depth depends mainly on the nature of the associated organisms, which are frequently dwarfed, and either blind or with enormously developed eyes, whilst that for deposition at a distance from land is confirmed by the ever-increasing number of cases of association of graptolitic deposits with others which are composed almost exclusively of tests of radiolaria. The most striking case of this has recently been detected by the geological surveyors amongst the rocks of the Southern uplands of Scotland (23). Messrs. Peach and Horne have there discovered beds with *Tetragraptus* of Middle Areing Age, separated from beds with characteristic Glenkiln (Upper Llandeilo) graptolites by a thin deposit of radiolarian chert. "We thus perceive that the great mass of strata which elsewhere forms the Upper Areing, and the Lower and Middle Llandeilo formations are here reduced to not more than sixty or seventy feet. Judged by the palæontological evidence these thin cherts appear to be a chronological equivalent of thousands of feet of ordinary sediment in North Wales. They, no doubt, were deposited with extreme slowness in a sea of some depth, and over a part of the sea-floor which lay practically outside the area of the transport and deposit of the terrestrial sediment of the time."

The graptolites are generally viewed as type-fossils of the Lower Palæozoic rocks, and this view is practically correct. The earliest graptolite which has hitherto been described, *Dichograptus? tenellus* Linnrs., occurs in the Lingula Flags of Sweden, below the shales with *Dictyo-*

*graptus flabelliformis* Eichw. which are so widely distributed. This *Dictyograptus*, by the way, which has a very limited vertical distribution, is probably in no way related to the long-ranged *Dictyonema.* Graptolites are extremely rare in the Upper Ludlow rocks, and have been detected in the Lower Devonian rocks of Bohemia, though it is doubtful whether their asserted occurrences in rocks of Devonian age in Scotland and the Harz Mountains are correct. It may be taken as fairly certain that they finally died out in Devonian times. Between the earliest and latest graptolitic deposits we have already a large number of graptolitic zones, which it will be of use to print in one connected list as this has not been heretofore done. So far as they have been made out they are, in ascending order, as follows: *Lingula Flags;* (i.) Zone of *Dichograptus? tenellus*, Zone of *Dictyograptus flabelliformis. Tremadoc Slates;* Zones of *Bryograptus. Areing Beds;* Zones of (i.) *Dichograptus*, (ii.) *Tetragraptus*, (iii.) *Didymograptus indentus var nanus. Llandeilo Beds;* (i.) Zone of *Didymograptus Murchisoni*, (ii.) Zone of *Cœnograptus gracilis. Bala Beds;* Zones of (i.) *Climacograptus Wilsoni*, (ii.) *Dicranograptus Clingani*, (iii.) *Pleurograptus linearis*, (iv.) *Dicellograptus complanatus*, (v.) *Dicellograptus anceps. Llandovery Beds;* Zones of (i.) *Diplograptus acuminatus*, (ii.) *Diplograptus vesiculosus*, (iii.) *Monograptus argenteus*, (iv.) *Monograptus convolutus*, (v.) *Cephalograptus cometa*, (vi.) *Monograptus spinigerus*, (vii.) *Rastrites maximus. Tarannon Beds;* Zones of (i.) *Monograptus turriculatus*, (ii.) *Monograptus exiguus*, (iii.) *Cyrtograptus Grayæ. Wenlock Beds;* Zones of various species of *Cyrtograptus* not yet fully worked out. *Lower Ludlow Beds;* Zones of (i.) *Monograptus bohemicus*, (ii.) *Monograptus Nilssoni*, (iii.) *Monograptus leintwardinensis. Upper Ludlow and Lower Devonian;* Zones of undescribed graptolites.

It is quite certain that this number will be very largely increased as a result of further work, but it is sufficient to show the importance of the Lower Palæozoic rocks when it is remembered that many of these Zones contain a fauna largely distinct from the faunas of the adjoining ones.

When the Zones are worked out more fully than is the case at present, we shall have a far better gauge of "Geological Time" than that founded upon the crude estimates made by measuring thicknesses of strata.

Lastly, the study of graptolites may possibly throw some light upon climatic change. I have already enlarged upon this elsewhere (24), and pointed out that the separation of graptolitic deposits from non-graptolitic ones amongst the Stockdale shales of the Lake District, the deposits themselves being lithologically similar, is most readily explicable by climatic change. The argument would be stronger had microscopic examination and chemical analyses of the strata been made, and I should be glad to supply any one who cares to look into this question, which is one of some interest, with material for such examinations.

In conclusion, the above notes will be sufficient to show the importance which the graptolitoidea have assumed not only to the geologist but also to the biologist. That they differ in any remarkable respect, as regards their teachings, from any other group of fossils is doubtful. Their special utility lies in the fact that owing to their characters they are preserved in sufficient numbers to allow collectors to obtain a large suite of specimens of almost every species with little difficulty; the result is that further advance has been made in their study than in that of many other groups which like them are only preserved in the fossil state. One word to the biologists. We are often told that fossils are of little use on account of the absence of soft parts, though biologists have not been much hampered by this when dealing with the Vertebrata. But to compensate for the want of soft parts, we are furnished with a countless supply of specimens whose order of appearance and disappearance we are able to a large extent to ascertain, and this is what the biologist can never obtain by confining his attention to recent organisms. From them he has been able to ascertain that evolution occurs; how it occurs is left for the palæontologist to describe. That the study of these organisms as pursued up to the present has not

been in vain, is conclusively proved by the best of all tests, namely, that we are able to predict the discovery of forms which are afterwards detected by the worker in the field, to whom we commend this group as one specially worthy of his attention.

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# INSULAR FLORAS.

## PART VI. (B).

IN my article (59) on the flora of the African Islands of the Indian Ocean, I dealt with the subject in considerable detail, but beyond the vascular cryptogams I had very few data concerning the Isle of Bourbon. Since then Dr. Cordemoy has published a *Flora* of the island (60), which is a consolidation of all the materials he has been able to collect during the leisure of upwards of thirty years' residence in the island, though unfortunately without a full collation with the rich earlier collections in the Paris Herbarium of Commerson, Du Petit-Thouars, and other botanists. Moreover, he has not worked out the geography of the plants to the extent he might have done, so that it takes some time to find and extract the particulars of special interest to the geographer. Indigenous and naturalised plants are included in the same enumeration without any typographical distinctions; and the summary is limited to a table showing the number of species of each natural order, including naturalised species. A rough calculation of the number of indigenous species of vascular plants, described or enumerated, gives a total of about 1100, whereof 200 are ferns, and 172 are orchids. This is nearly 250 higher than Baker's estimate (61) of the vascular plants of Mauritius; but, although the islands are nearly of the same size, the mountains of Bourbon rise to altitudes of between 9000 and 10,000 feet, or about 6000 feet above those of Mauritius; thus giving an additional climatic zone to the former island. And an analysis of the components of the flora shows that Bourbon possesses a much larger temperate element. But it should be known that Cordemoy takes a narrower view of species than Baker, especially in ferns; and some allowance would have to be made for this in comparing the totals. Apart from this divergence, the flora of the two islands is essentially the same, several genera and many species being common to both and found nowhere

else. The predominating natural orders of vascular plants occupy nearly the same positions numerically in both islands; ferns being first and orchids second, and Leguminosæ and Compositæ relatively low down; very different proportions from those obtaining in the Madagascar flora, in which these four orders occupy reversed positions. Thus: Leguminosæ, Filices, and Compositæ, followed by the Orchideæ, which are represented by just half as many species as the Leguminosæ.

The absence of a number of natural orders from Dr. Cordemoy's *Flora* that are represented in Mauritius may be accounted for partly by the fact that he did not work out the old collections made before the destruction of the virgin forests which formerly clothed the island. It is probable that many species have disappeared from both islands from the same cause. The following orders known to be, or as having been, represented in Mauritius are not included by Cordemoy: Xyridaceæ, Scitamineæ, Podostemaceæ, Myoporineæ, Bignoniaceæ, Lentibulariaceæ, Gentianaceæ, Rhizophoreæ, Connaraceæ, Simarubaceæ, Ochnaceæ, Burseraceæ and Nymphæaceæ. The absence of several of the foregoing orders might be accounted for without calling in the theory of destruction, but it would lead too far to attempt the discussion of the matter here. *Myoporum mauritianum* is an instance of a plant, and an order that is no longer represented, if it ever were; for there may have been an error in locality. The only specimen at Kew is labelled as coming from one small patch at the east end of the island of Rodriguez, which is some 300 miles distant from Mauritius. Moreover the Seychelles and Rodriguez between them possess several natural orders which do not reach Bourbon or Mauritius, though they are represented in Madagascar. They are Nepenthaceæ, Passifloraceæ, Turneraceæ, Dipterocarpeæ (?), Ternstrœmiaceæ and Dilleniaceæ; whereof the first and the fourth are essentially Asiatic, the second and third American, and the two last equally Asiatic and American. The parasitical Rafflesiaceæ are perhaps the only natural order in Bourbon that is not represented in Mauritius. Cordemoy records *Hydnora africana*

as common at St. Paul in Bourbon. It inhabits Eastern tropical and South Africa, though it is not known from Madagascar or any other of the African islands. Six or seven species of *Hydnora* have been described; all inhabiting Africa from Abyssinia and Angola southward to Cape Colony. I have previously noted (62) the discovery of a member of this order (*Cytinus Baroni*) in Madagascar. Since writing that I have seen a third Mexican species.

The intimate relationships of the floras of Bourbon and Mauritius may be gathered from the presence in the two islands, and restriction to these islands, of the following monotypic, mostly very distinct, genera: *Cossignya* and *Doratoxylon* (Sapindaceæ), *Grangeria* (Rosaceæ), *Roussea* (Saxifragaceæ), *Psiloxylon* (Lythraceæ?), *Fernelia* (Rubiaceæ), *Heterochænia* (Campanulaceæ), *Bryodes* (Scrophularineæ), *Monimia* (Monimiaceæ) *Dictyosperma* (Palmæ). To these may be added several other genera of the same geographical area, represented by more than one species; in five instances out of six by three species: *Fœtidia* (Myrtaceæ), *Pyrostria* and *Myonima* (Rubiaceæ), *Faujasia* (Compositæ), *Hyophorbe* and *Acanthophœnix* (Palmæ). Twenty-five other characteristic genera are restricted to the African region, using that designation in the sense of including therein the islands under consideration, Madagascar, and continental Africa. *Trochetia* (Sterculiaceæ) is remarkable among them as extending to St. Helena, where it is represented by two distinct species—or rather was, for one is quite extinct in a wild state. *Psiadia* (Compositæ) has the same range.

Allusion has been made (63) to the phenomenal concentration of endemic palms in the Seychelles, and it would be interesting to give the distribution and affinities of the palms of the whole of the East African Islands; but I must confine myself to the Bourbon species. The native species are five in number, namely: *Latania Commersonii*, *Hyophorbe indica*, *Dictyosperma album*, *Acanthophœnix rubra* and *A. crinita*. All these palms also inhabit Mauritius, and they are, so far as our present knowledge goes, confined to the island. All the genera are peculiar to this insular region if we take in Madagascar, and *Dictyosperma* and *Acan-*

*thophœnix* to Mauritius and Bourbon. *Latania* belongs to the Borasseæ and all the rest to the Arecineæ. As stated before, there is no parallel to this in insular floras of other parts of the world. Polynesia, both the eastern and western, is relatively poor in palms, and the West Indian Islands possess few endemic species; but, as explained a few pages back, Lord Howe Island possesses four endemic species of palms belonging to Australian and endemic genera.

Coming down to species we find that Cordemoy describes about 200 new ones, which, with those previously known as endemic, would make probably not less than 25 per cent. of the vascular plants endemic. It is probable that this number—the number of new species—may be subject to some reduction, especially in such groups as the ferns and grasses in which so many species have a wide range; yet 25 per cent. of endemic species is possibly below rather than above the mark. Nineteen grasses are described as new. Considering, however, the general distribution of grasses, and that only four species are regarded as endemic in Mauritius, there are good grounds for suspecting that many of the Bourbon species are not really new.

Orchids, epiphytal and terrestrial combined, contribute no fewer than seventy new species; and the total number of orchids thus exceeds the total indigenous species of any other two natural orders. In Mauritius, orchids are more numerous than any other order of flowering plants, but they only occupy the first place by a majority of about ten. As I have shown elsewhere (64) orchids are exceedingly rare or entirely wanting in oceanic islands, and such proportions as Cordemoy's enumeration gives would hardly be found in the richest orchid districts of Asia or America. Continental tropical Africa, so far as known, is relatively poor, whilst in Madagascar, according to Baron's tabulation (65), orchids stand third, being exceeded by Compositæ and Euphorbiaceæ. It is true that I have estimated (66) that orchids are numerically more strongly represented in British India than any other order of flowering plants, and my estimate has proved correct in the subsequent elaboration of this order (67) by Sir Joseph Hooker. It may be in-

teresting to add that orchids stand third in the flora of the whole world, and they also take the same position in the flora of Mexico and Central America.

Returning to the Bourbon orchids; the regional characteristic *Angræcum* is credited with eighteen new species, and a total of thirty-two species. There are also new species of the epiphytal genera *Bulbophyllum*, *Aeranthus*, and *Saccolabium;* but the bulk of the new ones are terrestrial plants, many of them very rare and inconspicuous, and most of them of short duration above ground.

On this point Cordemoy says: "J'en ai moi mème plusieurs nouvelles, en herbier, que leur mauvais état de conservation ne permet pas de décrire. Certainement il en existe d'autres non encore découvertes, surtout parmi les Ophrydées, dont plusieurs parcourent, en quelques semaines, la période active de leur vegetation, puis se replient immédiatement, pour passer le reste de l'année sous terre à l'état de tubercule. Plusieurs localités n'ont pas été suffisament explorées."

Three new genera of this group are described, namely, *Acrostylia*, *Camilleugenia* and *Hemiperis;* the first two being monotypic and the third having twenty-one species ascribed to it. All three would be included under *Habenaria* by some authors; but in this extended sense *Habenaria* is a vast and heterogeneous agglomeration of species.

Among other genera, of which several new species are described, I may mention *Dombeya*, *Evodia*, *Eugenia*, *Embelia*, *Sideroxylon*, *Geniostoma*, *Psiadia* and *Faujasia*.

In addition to the new genera of orchids, four others are proposed, namely, *Guya* (Bixaceæ), *Herya* (Celastraceæ), *Allocalyx* (Scrophulariaceæ), and *Mahya* (Labiatæ). According to the author's own admission, three out of the four are somewhat doubtful, and the affinity of the fourth is not given more definitely than by placing it in the tribe Mentheæ. But *Mahya stellata* is an interesting plant, whatever its affinity, because it is believed to be the only really indigenous member of the Labiatæ. It is a dwarf shrub, very rare, and found only near the summit of the Grand Bénard, at an elevation of about 8650 feet.

Strange to say the upper zone of vegetation is less alpine in character than that of the mountains of Madagascar and Tropical Africa. Cruciferæ, Caryophylleæ, Umbelliferæ, Primulaceæ and Gentianaceæ, as well as herbaceous Rosaceæ and Saxifragaceæ, are either exceedingly rare or entirely absent. In Madagascar, where the highest point is barely 8500 feet, the following familiar genera occur: *Ajuga*, *Alchemilla*, *Caucalis*, *Crassula*, *Drosera*, *Epilobium*, *Genista*, *Geranium*, *Linum*, *Pimpinella*, *Sanicula*, *Stachys*, and *Viola*, besides many others which are unknown among the native plants of Bourbon.

Gymnosperms are also unrepresented, both in the indigenous vegetation, and among the numerous naturalised plants. It is the same in Mauritius; but in Madagascar one species each of *Cycas* and *Podocarpus* has been discovered; the latter being prominent in certain districts.

Finally, I may add that the following orders are strongly represented in the Bourbon flora: Malvaceæ (*Dombeya*, 21 species, and *Ruizia*, *Astiria*, and *Trochetia*, regional genera); Rutaceæ (*Evodia*); Urticaceæ (*Ficus*, and *Obetia* and *Maillardia*, regional genera); Euphorbiaceæ and Convolvulaceæ.

Since the untimely death of Dr. H. Baillon another part (68) of the admirable illustrations of the flora of Madagascar has appeared. It consists largely of plates for intercalation, and the highest number is 340. Unfortunately no descriptive or explanatory letter-press has been published in connection with these plates and none is likely to be forthcoming. Surgeon-Major H. H. Johnston has published (69) an enumeration of plants collected by himself and regarded by him as indigenous in Mauritius, though they are not included in Baker's *Flora*. The total is fifty species, half of which are cellular cryptogams. There is nothing specially remarkable amongst them. The same gentleman has published an account (70) of the vegetation of the small islands in the Mahébourg Bay, Mauritius, namely: Ile de la Passe, Ile Vakois, Ile aux Fouquets, Ile aux Fous, Ile Marianne and Rocher des Oiseaux. These islands are of coralline limestone forma-

tion, and their flora is equally as poor, and composed mainly of the same species as the flora of the small coral islands of the Pacific Ocean, a specimen of which is given some pages back.

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[1] Since my remarks on the flora of Lord Howe Island were put into type, I have seen the "Macleay Memorial Volume," published by the Linnean Society of New South Wales, in 1893. It contains a paper by Prof. R. Tate "On the Geographic Relations of the Floras of Norfolk and Lord Howe Islands," in which the author arrives at much the same conclusions as myself. I had also overlooked a short article by Mr. C. Moore (*Transactions of the Royal Society of New South Wales*, v. (1872), pp. 29-34), on the same subject, but dealing only with the distribution of the genera. I may add that my detailed account of the vegetation and flora of the island has appeared in the *Annals of Botany* for June, 1896.

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# SELECTION IN MAN.

QUESTIONS respecting the origin and development of race-types have been among the favourite battle-grounds of anthropologists since anthropology began to be. Some have held that the countless varieties of type in man could be accounted for by the simple admixture of a very few original types, of three for example, a white, a black and a yellow one, others that nothing was needed to produce the widest extremes of variation save the direct influence of what the French call "media" and the Americans environment. With the development and increasing prevalence of evolutionary theories, the questions were looked upon from a somewhat different point of view. The same two parties, however, continued to exist, the one assigning supreme importance to innate variability controlled by natural selection, the other to the same variability controlled by environment. In process of time it became obvious that there might be other selective agencies than those commonly understood by the term natural; and Alfred Wallace himself pointed out that natural selection must have been potent in its working on man in the early stages of civilisation, but that in later stages it ceased to be so, while other agencies came into play.

Questions dependent on, or arising out of those already mentioned are innumerable, and in some instances at least are of obvious and immediate practical importance. For example: Which are the types of man that are most suitable for colonisation or acclimatisation in different parts of the world? and are they recognisable by colour or form of head, by kephalic or nasal index, by stature or any other visible character? What is the connection or relation, if any, between complexion and liability to malarial fever, to syphilis, to cancer or leprosy? Are the more fertile types or strains of mankind to be known by outward signs? Are new types of man likely to be developed more suitable than those now prevailing to the altering conditions of civic and industrial life, and if so, through what agencies?

Let us begin with the subject of complexion or colour, because it is one of the most conspicuous differential characteristics of man. The xanthochroic type of Huxley, the blond, at present so dominant and aggressive, occupying, in conjunction it is true with the melanochroic (or dark white), more and more of those parts of the earth, such as North America, Australia and South Africa, which have hitherto been the patrimony of the brown or the black man —is there reason to expect that it will hold its own outside of its original habitat, or even there?

The historical evidence is on the surface at least unfavourable. If we take the words used in their most natural sense, we must allow that the Greeks and Romans described not only the Germans but the Gauls and Thracians as blond. And they did not mean simply that the blond complexion was pretty common among these northern people; that could hardly have struck them as very remarkable; for if they had not had among themselves pretty frequent examples of it, their descriptions of the four temperaments could hardly be explained.[1] Literary portraits, and personal names such as Flavius, Rufus, Ahenobarbus, leave no doubt that there was considerable variety of complexion among the Romans of the republican period, though dark hues may have prevailed; and it does not appear that the continual influx of northern blood has been able to do much, if any, more than to maintain the status in that respect. The Greeks ascribed yellow locks to Achilles and Menelaus and other chieftains of the heroic age; but in the imperial age the Egyptian limners represented Greek ladies with black hair and eyes. The ballads of Mount Rhodope, believed to be of extreme antiquity, and referring to Philip, Alexander, and even Orpheus, ascribe yellow hair to their heroes; but the Pomaks of the Rhodope are not now a blond race.[2] Another argument may be derived from the ancient Egyptian wall-paintings. Not only

[1] Among the marks of the sanguine and lymphatic temperaments light hair is generally mentioned, while black hair belonged to the choleric and the melancholic.

[2] Fligier.

the Lebo or Tahennu and the Amorites (both probably enough of North-European origin, though domiciled in Lybia and Canaan), but some of the Arabian Shashi are represented as of xanthous complexion. Yet now-a-days we hear nothing of blonds in the Arabian or Egyptian populations, except where recent admixture of blood may be suspected. Again, Flinders Petrie's recent discovery of the remains of a tall, brown-haired, and apparently "Aryan" population in Middle Egypt,[1] that seems to have completely and speedily disappeared, reminds us of the generally accepted statement that the Mamelukes have no representatives in the Egyptian population at the present day. On the other hand the alleged blond coloration of the Guanches in the Canary Islands, and the known frequency of that complexion in the people of the Riff, and in the Kabyles of some mountainous regions further east, make it probable that the type of the Tahennu still exists where climatic conditions are not unfavourable to it. And after all, these Tahennu may have been only a blond military aristocracy ruling a melanochroic plebs; had it been otherwise, allowing that they had come from the north, why did they not perpetuate an Aryan language in North Africa?

Again, the large xanthous element in the Jews has been accounted for by the existence of an ancient Amorite cross; and on the whole this appears to me the most probable explanation. We can hardly doubt its antiquity in any case, since it is present in every section of the Jewish people, and is very distinct among the Sephardim of the Levant, though perhaps larger in proportion among the Ashkenazim,[2] whose Gentile neighbours are so largely blond. In some parts of the Levant, indeed, among the dark Turks and Armenians, a red beard raises a suspicion of Hebrew ancestry.

On the whole this kind of evidence, of which much

[1] "Indications of the earliest English occupation of Egypt," as De Lapouge pointedly remarked.

[2] Jacobs and Spielmann, *Anthrop. Trans.* Beddoe, *Ethn. Trans.*

more might be adduced, leads me to think that though selective agencies in the warm Mediterranean regions are on the whole adverse to the perpetuation of the blond type, they are not so everywhere or in very high degree.

Most of what evidence we have from northern countries makes one doubt whether any change has occurred except through immigration from melanochroic areas, and consequent admixture of blood. The Icelandic Sagas show that the Norsemen in the tenth century were as diverse in colour of hair as they are now; in fact the number of persons qualified as "black" would be a little surprising, if one did not allow for the probable inclusion of some whose hair was really only dark brown. The carefulness of the descriptions is vouched for by coincidences; thus, chiefs with a mixture of Irish blood, such as Skarphedinn and Kjartan, betray it by some Irish feature. The eyes are seldom mentioned; but Egil Skallagrimson, a pure Norwegian, had black eyes.

Similarly the old Irish poems and legends testify to the occurrence of the same varieties of complexion that now exist, and particularly to that of the very Irish combination of blue eyes and black hair, which is ascribed among others to the famous Diarmaid O'Duibhne, the semi-mythical ancestor of the Campbells.

Nevertheless I hold to the opinion, though only as an opinion, not as a firm belief, that the modern Norsemen are, if anything, more generally blond than their ancestors, and the modern Irishmen less so. If Scandinavia was, as now-a-days many think, the officina or breeding-ground of the blond long-headed type, may not the same agencies which worked in that direction after the close of the last glacial period be still operating there now, though it may be less powerfully? As for the Irish, it is certainly curious that no early English writer, so far as I am aware, makes mention of their dark hair. As I have said elsewhere, Giraldus tells us that the Welsh were of swarthy complexion, but he says nothing about the colour of the Irish (though he had much to do with them), except that incidentally and casually he says something about "long

yellow hair, like the Irish". The Irish colony about Dinas Mawddwy, in Merioneth, were called "the *red* men of Mawddwy". It is probable that the ruling tribes of Ireland had much more of the blond element than the servile ones;[1] and that the former were exhausted by the long wars with the English, by the military emigrations to France and Spain, and perhaps the earlier emigrations to America. Dr. Morton, the first great American anthropologist, in describing the Irish as he saw them, said "eyes and hair light". But there is no doubt that, speaking broadly, there is more dark hair in Ireland than in England or Scotland, though there are more dark eyes in England. The climate of Ireland, cloudy, moist and temperate, should favour the depigmentation of the eye by natural selection, and I have pointed out that the English colonists of Ireland by mixing their blood with that of the natives have changed their own type more in the direction of lightness of eye than of darkness of hair.

Mr. Galton has pointed out how rapidly a community in which the age of marriage is late would, under like circumstances, be crowded out or superseded by one in which that age is some years earlier. This consideration is one of several which account for the rapid extinction of upper class families in these islands, while the proletariat multiplies with inconvenient rapidity; and as the blond type is more prevalent in the upper than in the lower classes, it also is probably in process of diminution. If, however, it can be shown that the blond is more subject, *in this country*, to diseases of such a nature as to shorten life, and reduce the duration of the period of child-bearing and child-begetting, this same result would follow. Now there is a good deal of evidence as to the greater liability of blonds to certain classes of disease (in America at least), in Baxter's great work on the medical statistics of the Civil War. There are certain possible fallacies which may underlie Baxter's figures, to some of which De Candolle

[1] Thus MacFirbis, in a well-known passage, describes the Tuatha De Danaan as fair, and the Milesians as "white of skin, brown of hair," but the Firbolgs as a servile race, and black-haired.

has directed attention; but if we assume that the conclusions which result from them are at all approximately correct, it follows that the blonds in America have less chance than the brunets of contributing their due proportion to the next generation. Under these conditions the blonds ought to diminish relatively, and the brunets to increase; and accordingly we find that of accepted soldiers there were among the white natives of the United States about (per cent.)

| | | | | | |
|---|---|---|---|---|---|
| | | 66 light and | 34 dark complexioned, but | | |
| among the English | | 70 | ,, | 30 | ,, |
| ,, | Irish | 70 | ,, | 30 | ,, |
| ,, | Germans | 69 | ,, | 29 | ,, |

Thus the men of American birth yielded a larger proportion of brunets than those of any of the nations that had most largely contributed to their ancestry, which is nearly equivalent to saying that the Americans are more generally dark complexioned than their ancestors were. Gould (quoted by Ripley) found that the natives of the eastern states were also darker than those of the west. But whether this last fact is occasioned by the parentage of the western men being more directly European, or whether it is connected with the more migratory character of the blond type, must be left for the present undetermined.

Of European evidence on the relation of complexion and disease there is, so far as I am aware, no great amount. My own observations have shown that it is a mistake to suppose, as many do, that light-haired persons are in England more liable to phthisis than others. I have also pointed out that cancer is more common in persons of dark complexion, and in this I am supported by the observations of Dr. Roger Williams. This last fact has, however, very little bearing on the subject in hand, for as cancerous disease usually attacks persons who are beyond the child-producing age it can have very little effect on the proportions of the different complexions of the next generation.

As we possess for France not only elaborate recruiting statistics, with numerical lists for the principal disqualifying

diseases, but also Topinard's departmental statistics of colour, and Collignon's of head-breadth, and Bertillon's of mortality, one ought, it would seem, to acquire therefrom some solid grounds for the connection of physical types with disease, and for the estimation of their comparative liability, and of the probable results in the direction of selective propagation. In reality this turns out to be extremely difficult. "The prime difficulty" in such questions "is that these two factors, material prosperity and ethnic intermixture, in most cases follow the same laws of geographical distribution."[1]

Thus in France the conquering races, in most of which blond types originally prevailed, occupied, as a rule, the most fertile tracts, which were also generally the most level and those contiguous to the great ways of communication. It is in such tracts that civilisation usually progresses fastest, that great cities arise with their vices and sanitary disadvantages, and that blood is most mixed by continual migration and marriage. All these circumstances and conditions have to be taken into account before we can undertake to say anything as to the correlation of physical type with disease or military aptitude. The most promising plan seemed to me to be the throwing together of a number of departments having all *one* common character, but otherwise differing variously. The results thus gained are, however, more curious than conclusive. French anthropologists generally describe the tall, blond, long-headed type as subject to dental caries and myopia, and some add hernia to the list of its defects. Now the six departments, Nord, Pas-de-Calais, Somme, Aisne, Oise, and Calvados, which seem most distinctly to combine in their population all three marks of this type, have indeed a very bad record for dental caries, and, except Calvados, for general military unfitness; but three out of the six stand much better than the average of France as regards myopia and hernia. Moreover, bad teeth in the departments of France, strangely enough, usually co-exist with a low mortality, and I am

[1] Ripley, "Ethnic Influences in Vital Statistics," *Q. P. American Statistical Association.*

disposed to think that both are the outcome of some influences which increase in potency with the advance of civilisation. In any case the frequency of dental caries does not seem to have an unfavourable selective influence.

Phthisis, however, may and does have such an influence. And Houzé, having shown that it is more prevalent among the taller and fairer Flemings than among the shorter and darker Walloons, concludes that it has been the principal agent in producing the supposed reduction of the blond type in Belgium and elsewhere.

Now in England, as I have already stated, the proportion of blonds in the general population is quite as great as among the subjects of phthisis, but that of tall men among the phthisical is greater than that of short men. Let us see how it is in France.

"Pulmonary disease," "scrofula" and weak "constitution" seem to be so often confounded or interchanged in the recruiting statistics, that I have thought it advisable to class the three together, with the following results.

The three together are, or rather were in Boudin's time, the cause of rejection of conscripts in about the following order :—[1]

| | |
|---|---|
| 49 | in France. |
| 42 | ,, 10 most blond departments. |
| 39 | ,, 10 most brunet. |
| 41 | ,, 10 departments with tallest population. |
| 44 | ,, 10 ,, ,, shortest ,, |
| 54 | ,, 6 ,, ,, combination of stature, blond complexion and long head. |
| 48 | ,, 5 departments with combination of stature, blond complexion and long head, excluding the Nord. |

[1] This is not the correct way of putting it; but we have here the result of averaging the ranks in each of the three classes of disqualification, and counting each of equal value. In reality the number rejected for weakness of constitution is vastly greater than that for scrofula, and that again than for phthisis.

The low position of France as compared with her components is due to the greater and denser population of some of the worst departments, such as Seine and Nord.

| | | | |
|---|---|---|---|
| 43 | in 10 | departments | most long headed. |
| 43 | ,, 10 | ,, | most mountainous. |
| 48 | ,, 10 | ,, | most level but thinly peopled. |
| 52 | ,, 5 | ,, | most urban. |
| 49 | ,, 5 | ,, | Normandy. |
| 22 | ,, 4 | ,, | Brittany. |
| 45 | ,, 9 | ,, | with population of Auvergnat type. |
| 37 | ,, 5 | ,, | ,, ,, ,, Remolothringian type. |

Unquestionably the northern blond type does show badly here, but whether the blond complexion is much in fault is doubtful. The Remolothringian region (= Austrasia, or Champagne and Lorraine), which is one of the most blond areas in France, but brachykephalic, stands extremely well; in Brittany the Morbihan, the *most* blond department, stands best, and in Normandy the Orne, the *least* blond, stands worst.

The low position of the Nord may be compared with that of the ethnologically similar or almost identical Flemish zone of Belgium. Houzé himself ascribes this partly, but not, I think, wholly, to poverty, crowding, sedentary occupation, in fact to a number of causes outside of race.

Another method of inquiry suggests itself. If it be true that the blond type is more susceptible than the brown to the malign influences of urban life, and especially to phthisis, which is largely a disease of crowded city-dwellers, we should find this type less frequent proportionally than the brown in ancient cities. On this point we have a great deal of evidence; the greater part of this is supplied by the great inquest of Virchow into the colours of the school-children of Germany, those of Schimmer in Austria, of Kollmann in Switzerland, and of Vanderkindere in Belgium: we have also the observations on adults in Italy of Livi, and those of myself in the British Isles.

Georg Mayr, analysing the returns for Bavaria, pointed out that the town populations had on the whole a larger proportion of dark eyes, hair, and complexions than

the rural districts, and it appeared to him that this excess could not be accounted for by the larger proportion of Jews in the towns, as it occurred, though perhaps to a less extent, in places where the Jews were few.[1]

The subject has not been so carefully worked out for other parts of Germany; but a cursory examination of Virchow's figures shows that there is a larger proportion of dark hair in most of the great cities than in the surrounding rural districts, and this is more decidedly the case with the proportion of brown compared to blue eyes. Of 32 urban communities I find that in

11 the proportion of dark hair to fair is greater, and that of brown eyes to blue *much* greater than in the surrounding districts.
4 — of dark hair greater, of brown eyes greater.
4 — of dark hair greater, of brown eyes greater in less degree.
2 — of dark hair equal, of brown eyes greater.
2 — of dark hair less, of brown eyes greater.
4 — of dark hair about equal, of brown eyes about equal.
5 — of dark hair less, of brown eyes less.

These last are Halle, Wiesbaden, Krefeld, Ulm and Metz, most of which are towns which have grown rapidly of late. In the case of Metz the recent additions to the population have been derived from the blond region of Northern Germany. It may be noted that it is in that same blond region, generally speaking, that the most marked examples of the rule just laid down occur,[2] which fact strengthens the suspicion that the phenomena are largely due to the fact that the populations of these cities are partly constituted by immigrants of dark complexion from southern countries, including the Jews.

In Schimmer's Austrian statistics this last source of difficulty is avoided, the Jews being returned and classified separately from the Gentiles. Of 30 cities separately returned, 15 show a larger percentage of dark hair than their surrounding districts, and 14 a smaller one; in the remain-

[1] Thus it does appear in non-Semitic Nurnburg, though it is much more distinct in Semitic Furth.

[2] *E.g.*, Münster, Hanover, Altona, Berlin, Posen, Danzig, Elbing, Königsburg.

ing one, Linz, the proportions are identical. So far, then, there is blank disappointment; but when the eyes are examined the case is quite different: 27 cities show a larger proportion of dark eyes than their environs, and 3 only a less proportion.[1] In several of these 27 cases questions of race at once suggest themselves. In the Czechs, as in the Irish, the combination of light eyes with dark hair is common, while it is rare among the Germans. When, therefore, we find that in all the 6 cities of Moravia German is the school language, while in the country districts it is either Slavonic or mixed, and that in every one of these cities the eyes are darker and the hair lighter than in the surrounding districts, we need go no further for an explanation. But this will not serve in all the cases; and some probability remains that there is a certain kind of selection at work to darken the eyes of the urban population.

In Belgium the case is not so clear. Ghent, Antwerp, Ostend, and Verviers come out much darker than their neighbours; in most other cases the differences are slight either way. The cantons in Belgium are generally large, so that it is difficult to separate the urban and the rural populations. I have however picked out 11 cantons in which I think the urban element most greatly preponderates, and the results are as follows.

Of the 11, 9 have a larger percentage of dark eyes than the arrondissements to which they belong, 1 of lighter eyes; and in one, Mechlin, there is equality. But the hair, as in Austrian schedules, comes out about equal; in 6 of the towns it is darker; in 5, including Brussels and Liége, it is lighter. Ghent is, I suppose, the city in which the unfavourable selective influences of urban life (overcrowding, poverty, sedentary occupation, infectious disease, etc.) are likely to have been most intense.

In Switzerland Dr. Kollmann's schedules yield only two instances of a nearly pure urban community, Basel and

[1] Bozen, Bielitz, and Czernowitz (in the Bukowina): they are all comparatively small places, and all near to race frontiers, which may possibly account for the anomaly.

Geneva. Each of them is on a frontier, each is a singularly favourable specimen of a city, and is of little service for our purpose. Both Basel and Geneva have almost certainly a more blond population than that which surrounds them, whether Swiss, French, or German.

In the West of England, according to my own published observations on 3630 adults, mostly hospital patients, of whom 2486 were natives of towns, and 1144 of rural districts, the proportion of dark hair in towns was to that in the country, reckoning by the index of nigrescence, as 31 to 35; but that of dark eyes was as 58 to 49. We have here nearly the same phenomena as those we found to be so common in Germany, Austria, and Belgium.

In the British Isles generally, the drift of my own very extensive local observations (in which the place of birth however was never actually ascertained) was to show that in large towns, especially those with an old settled population, the darker colours both of hair and eyes were more prevalent than in the surrounding districts. This applied to the greater part of Britain, but in parts of the west where the native population is generally dark-haired, *e.g.*, Shrewsbury and Truro, the proportions may be reversed. The British military statistics, so far as investigated, *viz.*, to the number of 13,800 deserters, yield results similar, but not strongly marked. Thus London, Birmingham, Bristol, Newcastle, Brighton, and Portsmouth give an index of nigrescence of 8, against one of 4·9 for the rest of England; the proportion of dark eyes for the towns named being 39·5 per cent., but for the rest of England, 38·7. Edinburgh and Glasgow give together an index of 11·8, the rest of Scotland of 0·3 only, the percentages of dark eyes being 29 and 27·8; and Belfast and Dublin give an index of 18·7 against 15·2 in Ulster and Leinster, with percentages of dark eyes amounting to 32 and 28·4. The figures might be dissected with advantage, but to do so would lengthen this paper inordinately.

Livi's statistics as to this point are perhaps the most interesting, and have the advantage of being founded on the physical characters of adolescents (*i.e.*, conscripts). He

finds that fair hair is more uncommon and dark eyes are more frequent among the inhabitants of cities and their immediate vicinity than among those of the surrounding country. And this applies more or less to the whole of Italy, and cannot, therefore, apparently be accounted for by the immigration of the dark type from southern Italy into the northern cities, where the blond type is more common than in the south.

Thus I find in the northern and more blond region (Piedmont, Lombardo-Venetia, Liguria) 17 urban populations which, on a balance of eyes and hair, are darker than the rural populations around; 3 which are lighter, Brescia, Como, Rovigo; and 1, Verona, where the conditions are equal. In the central provinces, from Emilia to Campania inclusive, 19 cities are darker, 9 are lighter, and 2 are equal. In the south, including Apulia, etc., and the islands, where blonds form a very small minority, 11 cities are darker and 5 lighter. Thus in the north the rule obtains in 82 per cent., in the centre in 63, in the south in 69. The greater darkness appears to affect the eyes and the hair with something like equality, though not uniformly.

Livi, finding that the blond complexion is, with identity or supposed identity of race, more prevalent in the poverty-stricken mountainous districts than in the plains, and putting that fact into connection with its less prevalence in the cities, is disposed to consider it as connected with poor food and hard labour, which may retard development of pigment; in fact, he thinks the deposition of pigment to be an index of force and of development. Of course this is as yet unproven, and there is much to be said for and against the doctrine. But it does seem that we have evidence enough to show that in a great part of Europe the citizens are darker than the peasantry. This may be due to some direct influence of urban life, such as deficient oxygenation of the blood in children, but that seems very improbable. More probably it is due either to some kind of social selection such as Ammon and De Lapouge have studied, or else to the selection of the fittest for town life by the destructive agency of conditions more unfavourable to the

blond than to the brunet child. I propose to follow out the subject further in another article.

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# RECENT DISCOVERIES IN AVIAN PALÆONTOLOGY.

FOR reasons long ago pointed out by Lyell, fossil remains of birds are much more rarely found than those of other vertebrates, and, as a rule, occur in a very fragmentary condition. These circumstances, coupled with the difficulty in arriving at accurate determinations, owing to the great general similarity in the skeletal structures in most of the members of the class, have a direct bearing upon the scantiness of the results that have been attained in avian palæontology. In spite of these drawbacks, however, some not inconsiderable additions to our knowledge of fossil birds have been made during the last two or three years, and a short account of the chief papers on this subject may be of some interest. It will be convenient to take the papers roughly in the order of the geological age of the fossils they treat of, and to commence with those relating to the most ancient types.

*Pre-tertiary Birds.*—Unfortunately, with one exception, no remains of pre-tertiary birds have been discovered during the last few years. This is the more to be regretted because, interesting though many of the tertiary birds may be, they are in all essential respects similar to recent forms, and throw no light whatever on the mystery of the origin and early history of the group, the key to which lies buried in the Jurassic and Cretaceous rocks.

The single exception referred to is an imperfect tibia obtained at Judith River, Montana, from Cretaceous deposits of somewhat later date than those which formerly yielded the remains of *Hesperornis* and *Ichthyornis*. This tibia has been described by Marsh (1), who regards it as indicating a bird about two-thirds the size of *Hesperornis*, to which it is closely related, and has made it the type of a new genus, *Coniornis*, its specific name being *C. altus*.

*Tertiary Birds.*—In tertiary deposits of various ages and in widely distant localities, some important discoveries of bird remains have been made of late.

From the Eocene of New Jersey Marsh (2) has described some fragmentary bones which he considers belonged to a large struthious bird, *Barornis*, related to *Gastornis* and *Diatryma* from the Eocene of Europe and North America respectively. The specimens seem, however, to be too imperfect to admit of complete certainty as to the affinities of this bird, but it may be remarked that the "struthious" nature of *Gastornis* is very doubtful, though it was probably "ratite" in the morphological sense of that much abused term.

A portion of a metatarsus obtained in Vancouver Island from a deposit of Eocene or, at latest, Oligocene age, forms the subject of a memoir by Cope (3). This author, after an exhaustive comparison with recent types, comes to the conclusion that its affinities lie in the direction of the *Steganopodes*, and that of these *Pelecanus* is the nearest ally of the extinct form, to which the name *Cyphornis magnus* has been given. The presence of a large pneumatic foramen on the anterior face of the bone is strongly in favour of this view, and if, like the Pelicans, *Cyphornis* was capable of flight, it is by far the largest flying bird hitherto recorded.

A most important addition to our knowledge of the avi-fauna of the earlier tertiary rocks of Europe has recently been made by Professor Milne Edwards, to whom students of this branch of palæontology are already more deeply indebted than to any other writer. In a paper (4) read at the Ornithological Congress at Buda Pesth, he described a number of bird remains from the well-known deposits of phosphate of lime (Phosphorites) which occur in the neighbourhood of Caylus (Lot) in Southern France. The mammalian fauna of these deposits, described by Filhol and others, is an extremely rich one, and Lydekker has shown that several characteristic members of it occur at Hordwell in Hampshire in strata of Oligocene (Up. Eocene) age.

The birds now described belong to some seventeen genera, of which ten are new; these include representatives of several sub-orders. Only the more interesting of the new forms need be noticed here.

Of these perhaps the most important is *Archæotrogon*, which is closely related to the Trogons, and may indeed be an ancestral form of the genus *Trogon*, an extinct species of which has been recorded from the Miocene of Allier. At the present day these birds occur in the Neotropical, Ethiopian, and Indian regions; and it is remarkable that the extinct Miocene bird of Southern France should belong to a Neotropical genus rather than to one of those found in the Old World. This peculiar distribution of the recent and fossil forms is shown in a still more marked manner in the case of the next genus, *Filholornis*, which is said to be closely allied to *Opisthocomus*. The only known representative of this genus is the Hoatzin (*Opisthocomus cristatus*), which is one of the most peculiar and isolated forms of Carinate birds now living. It occurs only in Guiana and the Amazonian region, and is referred to a separate sub-order of which it is the only member. It is usually regarded as a primitive type, and the occurrence in Europe of a closely related bird is, therefore, another of those numerous cases in which such generalised types, now found only in the Southern hemisphere, have extinct representatives in the Northern. That the determination of the affinities of *Filholornis* is correct there seems to be no doubt, since Milne Edwards states that its ulna is almost a *fac-simile* of that of the Hoatzin, in which that bone is of a peculiar and distinctive form.

In the same memoir several new ralline birds are added to the already numerous rails recorded from the Tertiaries of France. One of the new forms, *Rallus dasypus*, though much smaller, is said to resemble *Ocydromus* in the form of its humerus; and another, *Elaphrocnemus*, a new generic type, approaches *Aphanapteryx* in the structure of its metatarsus. The occurrence in the lower Tertiary deposits of Europe of a large number of rails seems to be rather a strong argument in favour of a northern origin of the group, which, as Milne Edwards points out, is an extremely ancient one, of which at the present day we are only acquainted with some more or less degenerate descendants. Many of the more modified forms, such as *Ocydromus* and *Aphanapteryx*,

are now confined to, or have recently become extinct in, the Southern hemisphere. Between these and the primitive generalised rails there must have been many intermediate forms, one of which, in the opinion of Milne Edwards, is to be found in this new genus, *Elaphrocnemus*.

Other new genera of which the affinities are more doubtful are *Orthocnemus*, which resembles the Storks and Bustards in some respects and the Rails in others, and *Tapinopus*, which seems to have been a short-legged wading bird. We may also notice *Necrobyas*, a genus of owls presenting a combination of characters not found in any recent form; *Tachyornis hirundo* (previously described by Lydekker as *Ægialornis gallicus*), which is referred to the *Cypselidæ*; *Dynamopterus velox*, a cuckoo closely resembling *Eudynamis orientalis*, an inhabitant of the Austro-Malayan region; *Geranopterus*, allied to the Rollers and Momots; and, lastly, *Pterocles validus*, a sand-grouse considerably larger than any recent species.

Although many of the genera and species above noticed are founded on single or, at best, a very few bones, still in the hands of one so experienced in avian osteology as Professor Milne Edwards such material is sufficient for a fairly certain determination of the affinities of the fossil forms; and in this case the importance of the results from the point of view of geographical distribution cannot easily be over-estimated. It is much to be regretted that this valuable paper is not illustrated, since even the most careful descriptions of bird bones are very unsatisfactory without figures.

From the Middle Miocene of La Grive-St.-Alban in South-Eastern France, Lydekker (5) has described a small collection of bird bones. These, which do not include any very striking novelties, are referred to a new species of Owl, a large Pheasant, previously recorded by Milne Edwards from beds of about the same age at Sansan, a number of quail-like birds (*Palæortyx*), a Sandpiper and an undetermined Picarian bird.

The next addition to the ranks of fossil birds to be considered is by far the most important that has been

made since Marsh's discovery of the Toothed birds of North America. In this case Patagonia, a region long known for the wealth and peculiar character of its fossil Mammalia, has yielded a number of the most extraordinary avian types yet known. The discovery of these is due to Dr. F. Ameghino and his brother, to the former of whom we are indebted for the most complete account of them that has yet been published.

The first mention of the existence of gigantic extinct birds in Patagonia occurs in a letter from Carlos Ameghino, published in the *Revista Argentina de Historia Natural*, April, 1891, and containing a report of the results of his collecting expedition in Patagonia.

Some years before this (in 1887) F. Ameghino (6) had described under the name *Phororhacos longissimus* the symphysial portion of a large mandible which he considered to belong to an edentate mammal; a portion of a cranium, the type of the genus *Tolmodus*, was also referred to a member of the same class. In 1891, however, thanks to the new and better material obtained by his brother, he was able to show clearly (7) that both these specimens were in fact portions of the skeletons of gigantic birds, and to give a fairly complete diagnosis of the genus *Phororhacos*. In some points, as for instance in the statement that teeth were present, and that there was a bony helmet-like crest on the skull, this diagnosis, as Ameghino himself afterwards showed, is not quite correct; but it was the first definite statement of the chief characters of these extinct birds. The mandible was shown to be of enormous size and to curve upwards at its anterior end in a manner almost unique among birds; for though *Psophia* and *Dicholophus* were compared with it in this respect, they do not in fact possess this character. The upper mandible forms a strong hooked beak like that of a raptorial bird.

In the same year (1891) Moreno and Mercerat published a catalogue of the fossil bird remains in the La Plata Museum (8). This was illustrated by a large series of very beautiful photographic plates, but unfortunately these were unaccompanied by any adequate description of

the specimens. Several extinct penguins of the genus *Palæospheniscus*, from so-called Oligocene beds, as well as a number of Pleistocene bird remains, were figured in this work, but by far the most important section is that dealing with the great flightless birds of the Santa Cruz Beds. For the reception of these the authors established a new order, the *Stereornithes*, which was subdivided into four families, the *Brontornithidae* (including the genera *Brontornis* and *Rostrornis*), the *Stereornithidæ* (with *Phororhacos, Stereornis Mesembryornis* and *Patagornis*), the *Dryornithidæ* (with *Dryornis*) and the *Darwinornithidæ* (with *Darwinornis* and *Oweniornis*). *Psilopterus* (a name which, being preoccupied, was afterwards changed by Ameghino to *Pelecyornis*), a genus probably related to *Phororhacos*, was placed in the *Cathartidæ*.

At the end of the same year Ameghino published a synopsis of the South American fossil birds (9) in which he severely criticised the classification given above. He asserts that nearly all the new genera are merely synonyms of *Phororhacos;* the only exceptions being *Brontornis* which includes *Rostrornis*, and *Psilopterus* (*Pelecyornis*) which embraces *Patagornis.* Examination of the figures given by Moreno and Mercerat shows that in many cases, at least, he is right; for instance *Stereornis* is clearly the same as *Phororhacos.* On the other hand *Dryornis*, the sole member of the *Dryornithidæ*, is founded on the distal end of a humerus, which, judging from the figure, is probably that of a large vulture, most likely the Condor; it may be pointed out that this specimen is not from the Santa Cruz Beds but from a Pleistocene deposit.

In this paper Ameghino himself refers all these flightless birds to two families, the *Pelecyornithidæ* (including *Pelecyornis* and two new genera, *Lophiornis* and *Anissolornis*) and the *Phororhacosidæ* (with *Brontornis*, *Phororhacos*, and a new genus, *Opisthodactylus*). All these he regards as *Ratitæ*, and in this he was followed by Gadow (10) and Lydekker (11). Subsequently the latter of these writers, relying on the fact that the quadrate in *Phororhacos* pos-

sesses a double head for articulation with the skull, changed his opinion, and now considers them as degenerate Carinatæ in which the wing has been reduced in size.

Till recently our knowledge of the *Stereornithes* depended almost entirely on the preliminary notices of Ameghino, and on the plates of the catalogue of Moreno and Mercerat. At the beginning of last year however the former author published by far the most important contribution (12) to this subject that has yet appeared. He now described not only the specimens to which his preliminary notices referred, but also a large number of additional remains. The classification followed in this paper is different from that in his "Enumeracion," the order *Stereornithes* being adopted and subdivided into two families, the *Phororhacidæ* and *Opisthodactylidæ*. In the former *Pelecyornis* and *Lophiornis* are now included, while *Anissolornis* is considered to be a Gallinaceous bird: several new genera, some of which appear to be of rather doubtful validity, are also added. The *Opisthodactylidæ* include one genus only, *Opisthodactylus*.

Of the *Phororhacidæ* the skeleton of *Phororhacos inflatus* is by far the most completely known, the skull, mandible, pelvis, the bones of the fore and hind limb, and some vertebræ being described and figured. The skull is of a very remarkable appearance; from the side it most resembles that of a Raptorial bird, the enormous beak being sharply hooked at the anterior extremity, but when looked at from above it is seen to be much compressed, so that the premaxillary region, though very deep from above downwards, is extremely narrow from side to side. The quadrate has a double head for articulation with the skull, a character which, as Lydekker has pointed out (13), is opposed to the inclusion of these birds in the *Ratitæ*. The mandible is very heavily built, and its anterior end is curved upwards in a manner very unlike the ordinary avian mandible. The sternum is, unfortunately, quite unknown, but the coracoid and scapula have been preserved. The former is long and slender, quite unlike that of any Ratite bird; the acro-coracoid process is almost entirely wanting, and

the only avian coracoid which at all resembles the fossil in the form of its upper end is, I believe, that of *Aptornis*. The wing-bones are very small in proportion to the size of the bird, but, at the same time, are stout and strong; the ulna bears a number of tubercles marking the points of insertion of the secondaries. The pelvis is long and narrow, but in the posterior half, at any rate, it has been somewhat crushed, so that in fact it is broader than would appear from Ameghino's figure. The hind limb is long and comparatively slender; in the tibia there was a bony extensor bridge, and in the metatarsus the hypotarsus is simple. All the above details are taken from the skeleton of a single individual of the smaller species, *Phororhacos inflatus*, in which the skull is about thirteen inches long. In these birds the head is proportionately very large, and this species probably only stood about three feet high at the middle of the back. *Phororhacos longissimus* is about twice as large, the skull being two feet long and about ten inches high. Of the other genera *Pelecyornis* is the best defined, the pelvis and most of the limb bones being known. As already mentioned this genus was placed by Moreno and Mercerat among the *Cathartidæ;* and though there is little doubt that this is incorrect, it is by no means clear that Ameghino is justified in placing it in the *Phororhacidæ*, the pelvis being strikingly different from that of *Phororhacos* and the wing proportionately so much larger that it was probably still efficient as an organ of flight. The other genera of the family are for the most part known only from mere fragments of limb bones. *Brontornis* is a much larger and more heavily built bird than the largest species of *Phororhacos*, and *Opisthodactylus* is chiefly remarkable for the peculiar position in which Ameghino supposes the hind toe to have articulated with the tarso-metatarsus. In this paper also several extinct Penguins are described, as well as a number of ordinary Carinate birds belonging to several families.

Lydekker points out (14) that the age of the deposits in which these avian remains are found is probably much over-estimated by the South American writers, and that they are probably Miocene. He also discusses the relationship be-

tween the *Gastornithidæ* of the Eocene of Europe and the *Stereornithes* to which that family has been referred, and concludes that though it is not impossible that some affinity between them may exist, its nature is quite uncertain.

In a notice of the same memoir (15) the present writer has compared the skeleton of Phororhacos with several other types, and a considerable degree of resemblance with the Cariama (*Dicholophus*) was found to exist, particularly in the structure of the metatarsus. If further investigation of the specimens themselves should confirm these observations, the Cariama would appear to be related to these gigantic and highly specialised extinct birds somewhat as the recent Armadillos are to the extinct Glyptodonts. In both cases the recent forms cannot be regarded as direct descendants of the fossil giants, but rather as more generalised descendants from the same common stock, which have escaped extinction both on account of their smaller size, and more particularly, because being less specialised they were less affected by changes in the conditions of life.

The specimens described by Ameghino have been purchased by the Trustees of the British Museum, and many of them may now be seen at the Natural History Museum.

No papers of importance dealing with upper tertiary birds have appeared within the time to which this review is limited.

*Quaternary Birds.*—During the last three years some important additions to our knowledge of the extinct struthious birds of Madagascar, the *Æpyornithidæ*, have been made. Until 1893 only the bones of the hind limb and some imperfect vertebræ of these birds were known, and no paper describing new material had appeared since the publication of Milne Edwards and Grandidier's classical memoir in 1870. In 1893 Burckhardt (16) gave a very detailed account of a small collection of *Æpyornis* remains that had been obtained at Sirabé in Central Madagascar. This included not only limb bones and vertebræ, but also the greater part of the pelvis and sacrum ; all the specimens were referred to a new species, *Æ. Hildebrandti*, which the author compares with those previously known and with the

other *Ratitæ*. The conclusions he arrives at are of considerable interest. Milne Edwards and Grandidier expressed an opinion that *Æpyornis* is related to the *Dinornithidæ*, coming between that family and the Australasian Ratites, *Casuarius* and *Dromæus*, the latter of which according to some writers is the most primitive of the group. Burckhardt also considers that *Æpyornis* is most closely related to *Casuarius* and *Dromæus*, but believes that the resemblances between it and *Dinornis* are merely the result of parallelism in evolution, the skeleton in both cases having become extremely massive. On the other hand, he believes that in some of the characters of the pelvis and other parts of the skeleton, and also in the structure of the egg-shell, *Æpyornis* approaches *Struthio*, and suggests that from the primitive *Dromæus-Casuarius* stock the *Dinornithidæ* and *Apteryx* were descended on the east, while towards the west a branch arose which split up into the *Æpyornithidæ* and *Struthionidæ*. This view, though it may perhaps appear to be supported by the geographical distribution of the families concerned, cannot be regarded as established. The structure of the skull and shoulder-girdle and sternum when known will probably settle this question.

At the beginning of 1894 the present writer described (17, 18) a species of Æpyornis, *Æ. Titan*, far larger than any then known. The tibia is about thirty-one inches long, enormously massive, even more so than that of *Pachyornis elephantopus*. In January of the same year Milne Edwards and Grandidier (19) published a preliminary notice of a very large collection of *Æpyornis* remains. They name some four or five new species of *Æpyornis*, and establish a new genus, *Mullerornis*, for the reception of three smaller forms of more slender build than *Æpyornis*. A large part of the skeleton of one of the new species is very briefly described. The skull is said to be less flattened than that of *Dinornis*, and at the same time narrower and longer; the brain was proportionately considerably larger. The mandible somewhat resembles that of *Rhea*, while the sternum approaches that of *Apteryx* in structure. The coracoscapula is small, and bears a shallow glenoid cavity for the

head of the rudimentary humerus. Further descriptions and figures of this valuable specimen will no doubt be of great service in settling the question of the affinities of the family. The authors incline strongly to the view that *Æpyornis* is closely related to *Dinornis*, and, as in their former paper on this subject, suggest the former existence of a land connection between Madagascar and New Zealand to account for this relationship. In conclusion they state that there is clear evidence that *Æpyornis* was contemporary with man, and also mention that remains of a species of *Aphanapteryx* and a large extinct anserine bird occur in the same deposits.

In a later paper (20) the same authors describe in some detail the skull of one of the smaller forms included in the genus *Mullerornis*. This is said to differ widely from that of *Æpyornis*, the cranial region being much less depressed and the frontals raised so as to form a prominent boss. The basi-pterygoid processes are only slightly developed, and the anterior region of the premaxillæ is more compressed and forms a rounded keel above. Of all recent Struthious birds the Cassowary is said to most resemble *Mullerornis*, both in its cranial characters and in many points in the remainder of the skeleton.

An interesting account of the mode of occurrence of the bones and eggs of the *Æpyornithidæ* is given by Mr. J. T. Last (21), who resided in the island for some time and made collections in several localities. It appears that the bones are usually found in the dried beds of ancient lakes or in swamps, where they sometimes occur in large numbers; the eggs, on the other hand, are rarely found in such places, but occur in great quantities (in fragments) in the shifting sand-dunes round the coast.

In 1893 Professor Jeffrey Parker (22) published a new classification of the Moas, founded on the characters of the skulls. This paper is merely an abstract from his important memoir on the cranial osteology of the group, which will be noticed below. Here we need only mention that the *Dinornithidæ* were subdivided into three sub-families, the *Dinornithinæ*, *Anomalopteryginæ*, and the *Emeinæ*, and

that of all the genera *Mesopteryx* is considered to be the least specialised, and retains most nearly the ancestral characters of the family. At the same time it was also shown that some species probably possessed a frontal crest of large feathers, the points of insertion of which are marked by a series of pits on the cranial surface; in some cases this character seems to have been a sexual one.

In the same year Hutton (23) published a paper which may be regarded as an appendix to his important memoir, "On the Moas of New Zealand," which appeared in 1892, and consequently does not fall within the scope of the present review. In this appendix the author states that in his opinion it is necessary to subdivide the various genera of the Dinornithidæ into more species than had hitherto been done; since it is only by keeping the various species and varieties distinct, that the relative ages of the various superficial deposits in which their remains occur can be ascertained. The method of subdivision employed by him seems, however, to be open to the objection that it is an extremely arbitrary and artificial one, for in his former paper above referred to, as well as in the present one, he relies mainly, and in many cases entirely, on measurements of the long bones for separating the species. When we consider that it is possible to trace an almost complete gradation in size between the larger and smaller specimens of any given bone, it is clear that the number of species into which the series is divided, will depend upon personal opinion as to the latitude to be allowed for individual variation. In some cases where small differences in size, accompanied by other slight variations, are constant in two forms from different localities, the careful records and measurements given by Professor Hutton are of much interest and importance, but even in such cases it seems better to regard such small differences as indicating local races rather than distinct species.

Dr. H. O. Forbes (24) has severely criticised Professor Hutton's methods, and points out that in some cases the measurements given for one species fall within the limits assigned to another.

Casts of a number of pieces of limb bones of a small Moa, *Anomalopteryx antiqua*, which were discovered beneath a lava-flow at Timaru, are described and figured by Hutton (25). These specimens were first noticed by Forbes, who states that they were accompanied by remains of *Apteryx*. As to the age of the deposits in which these fragments occur there is much difference of opinion, and they have been successively referred to the Eocene, Miocene, Pliocene, and Pleistocene. Forbes believes that they are Pleistocene or at latest Upper Pliocene, while Hutton regards them as Miocene or Pliocene. In any case the specimens, which have been lost, were so imperfect that conclusions dependent upon them must be received with caution.

In a subsequent memoir (26) by the same author the structure of the axial skeleton in the various genera is discussed, and the descriptions of the various forms of pelvis and sterna are very useful, as also are the references to the published figures of various portions of the axial skeleton of the different forms.

Some ten years ago De Vis announced the discovery in Queensland of a femur of a species of Dinornis. The occurrence of the New Zealand type of Ratite bird in Australia would, of course, be a matter of great interest and importance in questions relating to the geological history of two areas; but the great difficulty in accurately determining isolated bird bones made it seem probable that in this case a mistake had been made. This suspicion would appear to be well founded, for Hutton, having had an opportunity of examining casts of the type specimen of the so-called *Dinornis Queenslandiæ*, states (27) that it differs widely from all Dinornithine femora with which he is acquainted. He considers that the bone is that of a bird related to Dromæus (the Emu), and coming between that genus and *Dromornis*, an extinct Australian form described by Owen.

The most valuable contribution to our knowledge of the morphology of the Moas that has been published for many years is Professor Jeffrey Parker's paper (28) on the cranial

osteology of the group. He has had opportunities of examining a very large number of skulls, some of which are those of young individuals in which the sutures are still open, and has, therefore, been able to give a very detailed account of the structure of this, the most important portion of the skeleton. Moreover, he has given a scheme of classification of the group, founded exclusively on cranial characters; the importance of this is obvious to any one acquainted with the terrible state of confusion into which, for various reasons, the nomenclature of the Moas has got. Five genera are recognised, and it would be very advantageous if these could finally be adopted, particularly as they agree in the main with those accepted by Lydekker in his catalogue of the British Museum collection, which, containing as it does the types of most of the species, must be the final court of appeal in most questions relating to the nomenclature of the family. Professor Parker has added a very detailed comparison of the Dinornithine skull with those of the other Struthious birds, and arrives at some interesting results as to the relationships existing between the various types. He considers that the Ratitæ are a polyphyletic group, Rhea and Struthio having originated independently of one another and of the forms inhabiting Australia and New Zealand. The latter arose from a common stock which early divided into two branches, the one giving rise to the Australian genera, *Dromæus* and *Casuarius*, the other to the New Zealand forms. The latter again divided into two branches, one leading to the *Apterygidæ*, the other to the *Dinornithidæ*. Of this family *Dinornis* and *Emeus* are regarded as having diverged most widely from the ancestral type, which is probably most nearly represented by *Mesopteryx*. This view differs from that of Burckhardt mainly in the refusal to admit a common ancestry of Struthio and the Casuariidæ, otherwise it is in general agreement with it, and is supported by the geographical distribution of the various forms. Unfortunately palæontology throws little or no light on the history of the Struthious birds, no fossil form that can be referred to that group with certainty being known from strata older than

the Pliocene. It is true that many extinct birds, as, for example, the *Gastornithidæ* and *Stereornithes*, have been referred to it, but in no case does it appear probable that we have to do with either actual ancestors or even offshoots of the ancestral Struthious stock. So far, therefore, as palæontology is concerned we have no means as yet of determining the relations of the Ratitæ either with one another or with other birds, and it is on such studies of the comparative anatomy of the various groups, as that given by Professor Parker in the case of the skull, that we must rely for information on this point.

Numerous papers have appeared lately dealing with the vexed question of the date of extinction of the Moas, and the points of view from which the problem has been attacked are very numerous. On the whole the evidence brought forward seems in favour of the view, so ably advocated by Dr. H. O. Forbes and others, that these birds have died out comparatively recently, and that their extinction is mainly due to the persecution they suffered from the Maoris, who hunted them down for food, and probably also destroyed their eggs. One of the reasons for believing that they survived till quite lately is the occurrence of portions of their bodies with dried flesh and feathers still adhering, several additional instances of which have been brought to light during the last year or so. Hamilton (29) has given a very interesting account of the various finds of Moa feathers, and more particularly of one which he himself investigated. In this case a large quantity of feathers, probably belonging to a species of *Megalapteryx*, were found in a cavern near the head of the River Waikaia, where a leg of the same bird with the flesh and skin still adherent had previously been discovered.

Some important discoveries of remains of extinct birds other than the Moas have been recently made in the New Zealand region. In a fissure in the limestone at Castle Rocks, Southland, Hamilton found an immense quantity of the bones of birds which appear to have fallen into the opening as into a pit-fall; though this can hardly have been the case with the large extinct eagles, *Harpagornis*, remains of both species of which occur. The remainder are nearly

all flightless forms, including *Anomalopteryx*, a large species *Fulica*, much like that found in the Chatham Islands (see below), a small Weka-rail, *Aptornis*, *Notornis*, and several others. An account of these, together with elaborate tables of measurements of the limb bones of some of them, will be found in Hamilton's paper (30).

The most important of all the recent discoveries in this region is, without doubt, that made by Dr. H. O. Forbes. In 1892 (32) he announced in *Nature* that he had received from the Chatham Islands (about 500 miles east of New Zealand) a skull of a large rail closely resembling the extinct *Aphanapteryx* of Mauritius; to this the name *Aphanapteryx Hawkinsi* was given. A large collection of bird remains, subsequently obtained from the same locality, contained all the more important bones of many individuals not only of this species, but also of several other extinct forms. Among the more notable of these were a large Coot, *Fulica chathamica*, very similar to the Mauritian species, *F. Newtoni*; a new type of Crow, *Palæocorax moriorum*, said to be most nearly related to the Gymnorhine group; an extinct Swan, *Chenopis*, besides several other species, most of which are still inhabitants of the Islands. Several of the extinct forms have not yet been described, but of *Aphanapteryx Hawkinsi* and *Palæocorax moriorum* a short account was published in the *Ibis*. At the same time a new genus, *Diaphorapteryx*, was established for the reception of the former species. Subsequently, however, the new name was withdrawn, and Forbes expressed his conviction that the Chatham Island and Mauritius birds are not generically distinct, and must, therefore, both be referred to *Aphanapteryx*. This opinion he defends in a short paper (34), illustrated by figures of the humerus, sternum, and premaxillæ of the two forms.

In a paper by the present writer (35), on the osteology of the Chatham Island bird, a number of differences between it and *Aphanapteryx broecki* are pointed out; and some of them, as, for example, the great dissimilarity between the metatarsi, are clearly of generic value, so that the name *Diaphorapteryx* was again adopted.

The assumed generic identity of these two forms was the most important new evidence brought forward by Forbes in his paper supporting the hypothesis of the former existence of an Antarctic Continent; but in the paper just referred to (35) it was shown that, as far as the birds are concerned, there is no evidence that the Chatham Islands have been united with any land area, and that the presence of two similar flightless rails on two islands remote from one another is no proof of any former land connection between them. In such a case it seems far more reasonable to suppose that both the islands may have been colonised by the same or allied forms of flying rails which have subsequently lost their powers of flight, owing to the very fact of their insular conditions of life. An instance of this on a smaller scale is found in the case of Tristan d'Acunha and Gough Islands, which are about 200 miles from one another and about the same distance from the Cape of Good Hope. Each of these islands is inhabited by a distinct species of Gallinule (*Porphyriornis*), which closely resemble one another and are incapable of flight; yet no one has suggested that on that account these islands were formerly united by land, either with one another or to Africa.

It is a fortunate coincidence that while the relationship between *Diaphorapteryx* and *Aphanapteryx* was still in dispute some additional remains of the latter were described. These bones, together with those of many other species, including the Dodo, *Lophopsittacus mauritianus*, *Fulica Newtoni*, etc., were described by Newton and Gadow in a well-illustrated memoir (36). Besides adding much to our knowledge of previously known extinct birds, the authors have been able to describe a number of new ones. They have also published a figure of the restored skeleton of the Dodo, which in several respects is more correct than those which have previously appeared. The whole of the remains described were obtained from the Mare aux Songes, from which previously a large quantity of Dodo bones had been collected. Besides the bones of birds those of the large extinct lizard, Didosaurus, and carapaces of Tortoises were found.

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## LIGHT AND ELECTRIFICATION.

### II.

*Continued from vol. iii., p.* 185.

WHEN we come to consider how to imagine the mode by which light discharges an electrified surface, one of the first hypotheses is that it may be by a kind of proof-plane action, the illuminated surface being disintegrated and its charged molecules evaporated away, taking their charges with them.

The first objection to such a hypothesis is that the disintegrating action of light ought to be otherwise perceptible either to microscopic inspection or to a delicate balance which should determine the loss of material.

It has, however, often been suspected that metals may evaporate more or less, and the fact of their smell seems to establish the fact, so it may be well to consider how small a loss of material will serve to explain the observed loss of electrification.

If we assume that each molecule so evaporated has the ionic charge on one of its atoms reversed, or, more simply, if we assume that each atom carries off a quantity of electricity of the order $10^{-11}$ electrostatic unit, its maximum possible and customary value, then the amount of electricity associated with the one gramme of evaporated silver is 900 contants or $3 \times 10^{12}$ electrodal units.

Now a silver plate 14 centimetres square, under certain conditions of arc illumination, was found in the writer's laboratory to lose negative electricity at the rate of 30 electrostatic units per minute when kept electrified to 80 volts in fairly free space. Hence the time that would elapse before the above plate would lose a tenth of a milligramme of its substance is ten million minutes or nearly a century.

Such a ratio of loss as that could not be detected by a balance, even in the case of silver, which is the substance most suitable for detecting a small electrolytic loss by weight. But now suppose that the discharge is not of so atomic a character, but that little flakes or pieces of the metal are driven off under the electric stress, so that the charge per gramme lost is very much less.

In that case the disintegration of surface might be perceived, but there are many difficulties in the way of supposing such an action.

The electric tension even when on the verge of disruption, when the surface is charged to many thousand volts, is by no means comparable to the forces of cohesion.

And the action of light occurs at so low a tension that it is impossible that its action is a mere bringing down of the limit at which disruptive discharge begins.

The action of light is much more like a quiet atomic or molecular process than it is like a disruptive discharge from the substance in bulk.

It may, however, be worth noticing that the electric repulsive force experienced by an atom *when on a surface charged to the disruptive limit* is not incomparably less than the average force of cohesion acting on such an atom. The tenacity of a metal may be taken as $10^9$ cgs. units, or about $10^{-7}$ degree, per superficial molecule. The electric force acting on an atom in a potential gradient of 30,000 volts per centimetre, which is the disruptive limit under ordinary atmospheric conditions, is about $10^{-9}$ degrees—one-hundredth of the average cohesive force; so it would not be unduly speculative to conceive it possible that circumstances connected with heat and other motors should occasionally

render individual atoms detachable under stresses approaching so near to the average limit, and this would be one way of representing disruptive discharge.

Against this, however, must be set the fact that the disruptive limit depends greatly on the atmospheric conditions, on the pressure and nature of the gas in contact with the metal; therefore it would appear that even for disruptive discharge we must look to an interaction between the molecule of the metal and that of the medium in contact with it, rather than to a simple disruption of the metal alone.

What is certain is that the charge is carried away by particles (atoms or otherwise) which travel along the lines of force to the oppositely electrified surfaces. It may conceivably be that the conveyers of charge are the electrons themselves; in other words, that the negative ends of the lines of force are detached from the charged body under the action of light, and that the line therefore promptly shuts up. It is more probable, however, there are no such detached electrons or atomic charges divorced from matter, but that the negative charge is conveyed by material atoms, whether they be the atoms of the metal or of the surrounding gas. To examine the question whether the conveying atoms belonged to the metal or to the gas, a number of experiments have been made in my laboratory with the object of testing the presence of metallic particles or vapour near an electrified metal rapidly discharging under the action of light.

The metals most easy to detect in small quantities are in general perhaps silver, iron and sodium. Silver, by its reflecting power when deposited upon glass; iron, by its magnetic properties; and sodium, by the light it causes a non-luminous flame to emit. Silver plates, with their clean edges opposed to the surface of plate glass, were oppositely electrified so that any charge given off from the silver edge should be deposited upon the glass as upon the dielectric of a Leyden jar, and were kept thus strongly illuminated by an arc light for hours; the glass was then examined for transparency. A decided deposit was found near the illu-

minated region, but there appeared nothing metallic about it, and it was easily dusted off. It seemed to be merely dust out of the air. So the experiment was repeated in a dust-free chamber, containing air filtered slowly through long tubes of cotton-wool, and now not the faintest local dimming of the surface could be observed, although the illumination and electrification lasted for days. So the answer for silver was in the negative.

Next a non-magnetic substance was hung in a powerful converging magnetic field in the neighbourhood of clean illuminated and oppositely electrified iron, to see if by condensation of evaporated iron, it was possible that it became magnetic. A minute torsion-bar of copper suspended over a clean, conical, vertically pointing electro-magnet's pole was the best arrangement. There were difficulties about this experiment on account of electrodal and other forces, but so far as disturbance could be eliminated the result for iron was also negative.

Then the most elaborate series of observation was made on metallic sodium kept in an atmosphere of highly purified hydrogen, the gas being supplied through a long series of drying tubes, and kept burning as a small jet just after it had passed over the sodium surface. By a mechanical arrangement the sodium could be cut to a clean surface from outside, and when the gas was pure this surface lasted a fairly long time, and under illumination it discharged electricity supplied by several dry piles in series, so that a considerable supply of electricity could be drawn from the flame whenever light from an arc lamp was allowed to fall on the sodium surface through a quartz window. The flame was looked at either direct or through a small spectroscope, and though the sodium line could not be kept wholly absent, its occasional presence depended in no way on whether the surface was positively or negatively or not at all electrified, nor on whether the light was or was not shining on it.

Hence I conclude that the discharge of electricity from illuminated surfaces is not effected by evaporation of those surfaces, but that the molecules which convey the charge

belong to something in the gas, and not to the illuminated body.

It may be asked whether dust in the air has any part in the action, but, so far as I can find, it has none at low tensions. The discharge rate from the silver surfaces, for instance, was just about as rapid in a dust-free atmosphere as when dust was present.

The proof that the discharge is effected by molecules of some kind, or at least by something which travels along the lines of electrostatic force was given by Righi. He electrified a small metallic cylinder of which only one generating line was free from varnish, and therefore clean enough to discharge electricity. This cylinder being negatively electrified in front of an earthed plate, an exploring terminal of an electroscope could ascertain which part of the plate was receiving a charge, as the cylinder was rotated on its axis, a movable slit being arranged in the plate for this purpose; and it was found to be always near one extremity of a circular arc of which the discharging line constituted the other extremity. He further found that if the illuminated body were free to move it receded like an electric windmill, proving that it had imparted its charge to something possessing appreciable inertia.

The inertia of the gaseous particles would indeed cause some divergence from the above circular orbits in which the electrical force is urging them, but the force is so great and the mass is so small that the deviation is not noticeable. Moreover, the charged atom has to make its way among a crowd of others by a process very similar to what occurs in electrolysis, so that the path of the electric charge follows almost accurately the line of electric force. In *that* sense it may be said to represent the motion of an electron or free electric charge, without committing the speaker to the hypothesis that such charges divorced from their usual boundary conditions on matter can really exist.

If a gaseous atom can receive a charge from an electrified surface there is no difficulty in understanding what it does with it, nor how, by such a process, the electrified body gets discharged, but the difficulty is to realise how an

atom can so receive a charge. Under ordinary circumstances it is certain that gas molecules cannot acquire a charge until the electrical tension rises to the disruptive point; but there is a certain condition into which a gas can be thrown, similar, if not identical, with that which chemists speak of as dissociation, wherein a gas becomes a conductor, that is to say, its particles do really act as carriers of electric charges, and may be spoken of as detached and specifically charged atoms.

Now in a vacuum tube, we learn experimentally from Mr. Crooks, that at high vacua the negatively charged atoms are vigorously repelled from a negative electrode, and, shooting out from it in straight lines, constitute what are known as cathode rays. It appears as if the electric discharge itself were carried on in a vacuum tube by a quiet, imperceptible, electrolytic action, originating at the anode or positive electrode, that this discharge fills the whole tube with positive electrification up to within a short distance of the cathode. In this short distance there is accordingly a steep potential gradient, and any stray negative atoms finding themselves therein are shot out of it with immense velocity, and constitute what are called cathode rays. Some doubt has been felt as to the essential nature of cathode rays, but there is hardly any good reason for the belief that they are anything else than a stream of negatively charged atoms of matter. They need not have recently received a charge, their charge may be intrinsic; what we observe is their repulsion, not as if guided through a resisting medium by electric force, but as if propelled violently inside a thinned layer and left to the first law of motion nearly.

Great interest has been felt in this cathode stream for a quarter of a century, but within the present year its importance has become immense owing to the discovery of Röntgen that a surface on which the stream impinges becomes capable of emitting a novel kind of radiation which travels even more persistently in straight lines, and is not readily stopped by material obstacles. This discovery must ultimately throw a great deal of light upon the whole

subject, and it is over soon to attempt to forecast its probable development; nevertheless a partial attempt may be made for what it is worth.

The new radiation appears to differ from ordinary ultra-violet radiation only in the matter of wave-length. Its wave-length is probably extremely short, not vastly greater than the size of atoms, and all its other known properties and peculiarities will follow from that according to known theories of dispersion, especially the electro-magnetic one of von Helmholtz.

Now this X radiation, when it falls upon an electrified surface, discharges it, somewhat in the same fashion that ultra-violet light does; but whereas light discharges electricity solely, or at any rate chiefly, of the negative sign, this X radiation discharges both positive and negative; and indeed it seems to act by converting the gas or other insulating material near a charged body into a conductor. This it probably does by dissociating the substance into charged atoms which are then free to act as carriers, and speedily convey to a distance the charge of the electrified body by journeys along the lines of force.

It may be that ultra-violet light acts in somewhat the same way, but not in exactly the same way. The air is transparent to ultra-violet light, it is not perfectly transparent to X rays.

There is no difficulty in supposing that the X rays dissociate some ingredient of the atmosphere, but there is great difficulty in supposing ordinary ultra-violet light to be able to do so. What the ultra-violet light chiefly does is to promote or to create the conditions necessary for the ready interchange of electric charge between gas and solid; and that this is so is practically proved by the great importance of the nature of the solid surface, as well as of the gas in contact with it. The gas seems indeed of secondary importance, but the cleanness and oxidisability of the solid is essential to a rapid and ready discharge with ordinary light from the visible spectrum. High ultra-violet light can act indeed over a wider range, and whereas light of long wave-length can only discharge negative

electricity, it is probable that light of extremely short wave-length can discharge positive also, and from surfaces not specially clean nor oxidisable.

The X rays seem to go farther in the same direction; that is to say, their activity does not appear to depend much upon the nature of the surface, nor do they seem to discriminate much between positive and negative electrification.

We may surmise, then, that long-wave light is effective in promoting discharge only when dissociated or incipiently dissociated atoms are already present in the neighbourhood of the surface. It is otherwise known that strongly electro-positive substances, like clean sodium or zinc, are surrounded by a number of electro-negative (chiefly oxygen) atoms, straining to get at it. And, similarly, a negatively charged surface may be surrounded by a number of straining positive atoms. Under these circumstances it is not difficult to picture the result of impinging waves of light, and of the electrical oscillations which they must necessarily set up, as resulting in an interchange of electricity between the surface and the gas which otherwise might not have occurred.

When positive electricity has thus been received by the metal from the air under the action of light, detached negative ions will be left in the atmosphere, and these will be repelled by the body if kept negatively electrified, and so may constitute a kind of feeble cathode ray.

Thus it appears as if there were a sort of reciprocal action; the impact of light on a negatively electrified surface results in the production of something akin to cathode rays, and the impact of cathode rays upon a positively electrified surface results in something akin to light.

Another instance of reciprocity has also been observed. Certain substances exposed to X rays fluoresce strongly, that is, emit light which in some cases persists an appreciable time, and some of these substances when made to fluoresce by exposure to light begin to emit X rays and continue to emit them for long after, as has been observed by M. Becquerel.

There is one matter dealt with in the last article which requires more cautious handling than it then received, and

that is the discharge of positive electricity—*i.e.*, the reception of negative by certain substances. It is a phenomenon which undoubtedly occurs as an experimental fact, but if we proceed to look into the cause of it we find its detailed character by no means so obvious Certainly it depends a great deal on the surroundings, and there is reason to believe that if a positively charged body were surrounded by a surface incapable of giving off negative electricity, then the apparent discharge of positive might not occur.

The question is complicated by the simple facts (*a*) that we cannot have a charged body without an equal opposite charge on surfaces opposed to it, and (*b*) that every surface reflects and scatters some of the incident light which therefore partly falls upon the oppositely electrified surface. Hence when a positively charged body loses its charge, it may be not through a direct action of the light upon itself, but by reason of the action of the reflected and scattered light on the negatively electrified surfaces in its neighbourhood.

On this hypothesis a surface which appears to lose positive more quickly that negative is one which of itself hardly loses any electricity at all; it loses negative slowly but it is exposed to surfaces which can emit it more quickly, and hence when it is positively electrified and they are inductively negative, it receives from them a negative charge more rapidly than it was able to give one out.

A large number of experiments have been made in the writer's laboratory to test this point, mostly by means of regular reflectors so as to avoid scattered light as far as possible; the details are somewhat technical and troublesome, and the very dust of the air is apt to scatter a good deal of active light; but the result is, on the whole, to substantiate the above-mentioned idea, which also possesses the powerful support of Messrs. Elster and Geitel, that the loss of positive electricity under the action of light is an indirect and secondary phenomenon.

It appears, however, that under X rays both points of electricity are discharged equally, and if these X rays are, as everything now indicates them to be, an extension into

very much higher regions of the spectrum of transverse ethereal vibrations, then it must become a question of degree and of wave length, as implied above by the writer, and no perfectly simple statement can be made.

The activity of ordinary sunlight in promoting the discharge of electricity into the atmosphere is evidently a question of great meteorological importance ; but it is enormously affected by the condition of the earth's atmosphere, At high elevations the rays are very active, but in valleys the power is less, and on many days in a town there is hardly any power left at all. The writer's assistant, Mr. Davies, constructed a portable apparatus with which he made many observations in Wales and other places during last summer at different heights and at different periods of the day. The results are such as might naturally have been expected, but we do not yet know whether the sun emits any X rays at all detectable in the higher region of the atmosphere, or whether this latter variety of radiation is an artificial product recently introduced by man into the operations of Nature.

OLIVER LODGE.

# AN EXTINCT PLANT OF DOUBTFUL AFFINITY.

IN two previous articles[1] some account has been given of the genus *Sphenophyllum*, with special reference to the structure of the strobilus. I now propose to add a brief summary of our knowledge of this interesting type of extinct plants, which has been fully dealt with by Williamson and Scott in their memoir on *Calamites*, *Calamostachys*, and *Sphenophyllum*.[2]

Every collector of Coal-Measure plants must be familiar with the fragments of slender stems bearing regular whorls of wedge-shaped leaves, which are frequently found in the Upper Carboniferous shales, or in the ironstone nodules of Coalbrookdale and other places. Writing in 1822, Brongniart[3] describes and figures a well-preserved impression of a species of *Sphenophyllum* under the name *Sphenophyllites emarginatus*, and speaks of it as a plant without any living generic analogue. In the classic *Prodrome d'une histoire des végétaux fossiles*, the same author gives the following definition of this fossil genus, and adopts the generic name *Sphenophyllum*[4]:—

"Tige simple, articulée ; feuilles verticillées, au nombre de six à douze, distinctes jusqu'à leur base, cuneiformes, entières ou émarginées, ou même bifides, à lobes plus ou moins profondément laciniés, presque dichotomes. Fructification inconnue." It is unnecessary to give any historical sketch of the various opinions expressed by later writers on the nature of this characteristic plant, but we may at least point out, that it has been held by certain authors that the plant regarded by Brongniart and others as an autonomous genus, was in all probability a particular form of calamitean branch. Stur was one of those who held this view, and in

[1] "SCIENCE PROGRESS," vol. i., p. 54, and vol. iv., p. 261.

[2] Williamson and Scott.

[3] Brongniart (1). Pl. xiii., fig. 8.

[4] Brongniart (2), p. 68.

his great work on *Calamites*, several specimens are figured and described as evidence of the calamitean nature of *Sphenophyllum*. A restoration of *Calamites* with sphenopylloid and other branches, given in his monograph, serves to illustrate this view.[1] More recent investigation has, however, conclusively proved that Brongniart's original definition holds good. There can no longer be any doubt that *Sphenophyllum* is a very well-defined generic type holding a somewhat isolated position in the plant kingdom.

From structureless casts and impressions, we learn that the genus is characterised by a comparatively slender articulated stem bearing a series of superposed whorls of leaves. The number of leaves in each verticil is always some multiple of three, frequently six, or it may be nine, twelve, eighteen, or more at each node. The leaves have usually a wedge-shaped form, and the lamina is traversed by dichotomously branched veins. In older forms, again, the leaves are much narrower, and each segment in a whorl has a single median vein. The narrow-leaved species, such as *Sphenophyllum myriophyllum* Crép., etc.,[2] cannot always be readily distinguished from the well-known *Asterophyllites* form of foliage; but as Zeiller[3] points out, a careful attention to the general habit of the plant, and the presence of bifurcations in the leaves, should enable us to separate these two generic forms. Another feature worthy of note is the hetrophylly occasionally exhibited by this genus.[4] The occurrence on the same plant of broad and finely dissected leaves, naturally suggested to some authors[5] the idea of an aquatic plant; but the histological features are not such as are usually associated with water plants.

Examples of *Sphenophyllum* met with in English Coal-Measures do not, as a rule, attain any considerable length. By far the longest stem which has come under my notice is one in the Geological Survey Museum in Vienna; in this specimen there is an axis 4 mm. in breadth with a length of 85 cm., giving off a slender branch 61 cm. in

[1] Stur, p. 69. [2] Zeiller (1), pl. lxii. [3] Zeiller (2), p. 674.
[4] Schenk, pl. xliv., fig. 1; Seward, p. 3, fig. 1. Etc. [5] *E.g.*, Newberry.

length. Occasionally long and narrow strobili are found attached to the vegetative branches; in external appearance they resemble to some extent the corresponding structures in calamitean plants, but a closer inspection at once reveals a very distinct individuality for this type of strobilus.

In Williamson and Scott's work three specific forms of Sphenophyllum are dealt with. We may first of all give a short description of the general type of structure characteristic of the genus, and afterwards attempt a diagnosis of the specific characters.

*Primary Structure of the Stem.*—Traversing the young stem there is a single vascular cylinder or stele, consisting of a triarch and centripetally developed axial strand of xylem. A transverse section of such a stem shows in the centre a triangular group of reticulate, scalariform, and spiral tracheids, the latter having a smaller diameter than the others, and constituting the three protoxylem groups at the prominent angles of the solid vascular axis. It is a fact of considerable interest, that we have in this primary structure an arrangement and manner of development of the tracheids which a student of Botany is always taught to regard as characteristic of root rather than stem structures. External to the xylem there is occasionally preserved a thin-walled phlœm tissue, and beyond this may be recognised the pericycle or limit of the stele. Passing beyond the central cylinder we have a thicker walled cortex, of which the outermost layer or epidermis has not been clearly preserved.

*Secondary Structure.*—On examining a series of transverse sections of stems in different stages of secondary growth, we find that the triangular group of primary tracheids becomes gradually surrounded by radially disposed rows of large elements, forming in older stems a considerable thickness of secondary wood, in which, as a rule, there is a striking uniformity in the diameter of the tracheæ. Smaller xylem elements occasionally occur, but, as in the majority of Coal-period plants, there are no definite rings of growth. The development of secondary xylem, beginning in the interfascicular region, that is, in the broad

bays of the primary wood, soon extends to the fascicular regions, and thus completely encloses the axial strand. The amount of secondary wood naturally varies considerably in different sections, the tracheids in a single radial row varying from one to thirty-seven in number. The medullary rays either extend as continuous lines of parenchyma through the whole thickness of the wood, or occur in the form of cell groups at the angles of the tracheids; in the latter case the apparently isolated clusters of parenchyma are united by connecting cells stretching across the radial walls of the reticulately pitted tracheids. Owing to the smaller diameter of the fascicular tracheæ, the secondary xylem exhibits a fairly obvious division into six groups, three broader masses of interfascicular tracheids, alternating with three smaller groups of radial rows of fascicular tracheids, tapering towards the protoxylem angles of the primary xylem.

The formation of periderm is another characteristic feature in the secondary growth of a *Sphenophyllum* stem. A phellogen or cork cambium appears to arise in the pericycle, and at a later stage the phlœm parenchyma takes part in the development of cork tissue. It is often a matter of some difficulty to distinguish between the true phlœm and the internal periderm. The latter consists of short cells in regular series, the former being made up of much longer elements, which may possibly be sieve-tubes.

*Leaves.*—The most perfect example of a petrified leaf of *Sphenophyllum* so far described, is one figured by Renault.[1] In transverse section the lamina is seen to be composed of thin-walled loose parenchyma, with small groups of tracheids marking the position of the veins. The epidermal layer on the upper and under surface consists of fairly thick-walled cells, with indications of stomata. The most distinctly preserved stoma has, however, been figured by Solms-Laubach[2] from the epidermis of one of the leaf segments of a strobilus; in this there are two narrow guard cells with two larger subsidiary cells.

[1] Renault (1), pl. ix., fig. 6; and (2), pl. xvi., fig. 1.

[2] Solms-Laubach, pl. x., fig. 9.

*Root.*—As regards the roots of this genus we have but little information. Renault[1] has figured a small silicified example from Autun, with a diameter of 2 mm. In the centre there appears to be a diarch primary xylem bundle, surrounded by concentric rows of reticulately pitted tracheids. It is possible that two specimens figured by Felix,[2] may represent adventitious roots being given off from a *Sphenophyllum* stem. He speaks of them as examples of lateral branching, but their precise nature is, by no means, very easy to determine.

*Fructification.*—The fructification of *Sphenophyllum* as first described by Williamson and Zeiller,[3] may be thus defined :—An axis traversed by a triangular strand of primary xylem tracheids, bearing at intervals of 1·5 to 2·5 mm. similar leaf verticils consisting of a number of linear lanceolate segments, fused in their lower portions into an open funnel-shaped disc. The numerous sporangia occur in 2 to 4 concentric circles on the upper surface of each disc, in radial sections of a cone presenting the appearance of a row of 2 to 4 sporangia between each whorl of bracts. Each sporangium is attached to a slender stalk springing from the upper surface of the leaf disc, and terminates in a hooked tip facing the axis of the strobilus, thus resembling the attachment of an anatropous ovule to its funicle. Each sporangiophore possesses a strand containing a few xylem tracheids. At the point where the stalk or sporangiophore passes into the sporangium, the epidermal cells have thicker walls, and appear to represent an annulus, the sporangia dehiscing by a longitudinal slit on the side away from the stalk. The sporangia are isoporous, and the spores have a reticulately marked outer membrane.

In a recent paper by Count Solms-Laubach[4] an exceedingly interesting addition is made to our knowledge of the *Sphenophyllum* strobilus. While confirming in the main the results arrived at by Zeiller, Williamson, and Scott, he describes a new type of fructification from the neighbour-

[1] Renault (1), pl. viii., fig. 5; and (2), pl. xv., fig. 6.

[2] Felix, pl. vi., figs. 2 and 7.

[3] See "Science Progress," vol. i., p. 54.

[4] Solms-Laubach.

hood of Cracow. In this species, *Bowmanites Römeri*, Solms, the sphorangiophores springing from the upper surface of each whorl of bracts, bear at the apex two sporangia instead of one as in previously known forms. Between each pair of sporophyll verticils there are at least three whorls of sporangia, the sporangia are almost sessile, and attached to short sporangiophes in the same manner as in the fructifications already described. Each sporangiophore bifurcates towards the distal end, and the sporangia are attached to the diverging forks in much the same manner as the ovules of *Zamia* and *Encephalartos* are suspended from their carpophylls. As regards the nature of the spores and the annulus-like cells of the sporangial stalks, *Bowmanites Römeri* agrees closely with the other forms. As happens so frequently in palæobotanical research, we are able to examine in detail the characters of an isolated member of an individual plant, without knowing anything of the other parts of the same species. In the present instance we are ignorant of the nature of the leaves which were borne by the stem to which *Bowmanites Römeri* was attached. There can, however, be little or no doubt that we have to do with a *Sphenophyllum* strobilus, differing in an important respect from the ordinary type. The plants included in the genus *Calamites* are known to have possessed cones of more than one type of structure; and it would appear that this was also the case with *Sphenophyllum*. When our data are more complete it may be possible to institute new generic terms for plants which are now assigned to these somewhat comprehensive genera, but for the present it is better to err on the side of too wide a meaning for generic terms, than to attempt to found new genera on insufficient evidence.

In addition to a full account of *Bowmanites Römeri*, Solms discusses at some length another sphenophylloid strobilus originally described by Weiss as *Bowmanites Germanicus*[1], and suggests that this species as well as that described by Binney under the name of *B. Cambrensis*[2]

[1] Weiss, Pl. xxi., fig. 12. Solms-Laubach, Pl. ix., fig. 7.

[2] Binney, Pl. xii., figs. 1-3.

may be identical with *Bowmanites Dawsoni* of Williamson.

It remains for us to consider the probable systematic position of this genus. It is undoubtedly a Vascular Cryptogam, characterised like so many other Palæozoic representatives of the group by a considerable development of secondary xylem and phloem.

Zeiller has expressed the opinion that *Sphenophyllum* should be included in the *Filicinæ*, and in the neighbourhood of the ferns; he institutes a comparison with *Marsiliaceæ* and *Ophioglosseæ*. The French author draws special attention to the distinct resemblance between the sporangiophore of *Sphenophyllum* and the sporocarp stalk in *Marsilia*. Subsequent writers have very properly pointed out that we cannot well make use of this superficial resemblance, in attempting to discover characters of real morphological importance. The single sporangium of *Sphenophyllum* differs in an important degree from the elaborate sporocarp or highly specialised foliar structure of *Marsilia*. The fossil genus is no doubt eusporangiate, and in that respect comparable with *Ophioglossum*; but the fertile spike of the latter differs widely from the sporangia and sporangiophores of the former. Potonié's[1] comparison of *Sphenophyllum* with *Salvinia* does not render any material assistance to our endeavours to assign the fossil form to its true position. "We must be content for the present to leave this remarkable genus in its isolated position, in the hope that the extensive knowledge of its organisation which we now possess may in the future afford an adequate basis for comparison, when additional forms of Palæozoic Cryptogams shall have been brought to light."[2]

This conclusion arrived at by Williamson and Scott, and accepted by Solms-Laubach, may perhaps be best realised by making use of the term *Sphenophylleæ* as a class designation. This has been done by Schenk in Zittel's *Handbuch der Palæontologie*,[3] and is the course followed in a recent paper by Kidston[4].

[1] Potonié.
[2] Williamson and Scott, p. 946.
[3] Zittel.
[4] Kidston.

## PTERIDOPHYTA.

### Class—SPHENOPHYLLEÆ.

Genus SPHENOPHYLLUM.—Brongniart 1828 (*Spheno-phyllites*, Bronguiart, 1822). Stems comparatively slender (1·5 to 15 mm.?), articulated, usually somewhat swollen at the nodes, and marked by more or less distinct ribs and grooves which do not alternate at the nodes, occasionally a single branch given off at a node. Leaves in verticils, usually the leaves of each whorl are equal in size, but may be unequal,[1] in multiples of 3, 6, 9, 12, 18 or more. The leaves of successive whorls superposed, not alternate; varying in form from cuneate, with narrow base and multinerved lamina having an entire or toothed anterior margin, to narrow linear uninerved forms, or with a deeply dissected lamina having dichotomously branched segments.

Stem monostelic, with a triarch triangular strand of centripetally developed primary xylem, consisting of reticulate, scalariform and spiral tracheæ; the protoxylem elements being situated at the blunt corners of the xylem strand, from the angles are given off the foliar bundles, either one or two from each angle.

Secondary xylem consists of radially disposed reticulately pitted tracheæ, developed from a cambium layer. Phloem of thin walled tissue including sieve-pitted tube-like elements and phloem parenchyma. Both xylem and phloem traversed by medullary rays of parenchymatous cells. Cortex largely composed of fairly thick walled cells; and in older stems cut off by the development of deep-seated periderm.

Fructification in the form of long and narrow strobili, in some cases reaching a length of 12 cm., and a diameter of 14 mm. A slender axis bearing whorls of numerous linear lanceolate bracts fused basally into a coherent funnel-shaped disc, bearing on its upper surface sporangiophores[2] and

[1] Zeiller (2), p. 675.

[2] The strobilus of *S. trichomatosum*, Stur, figured by Kidston, is described as having *sessile* sporangia. On this point see Williamson and Scott, p. 942. An examination of Kidston's specimen certainly conveys the idea of sporangia without stalks, but the evidence is not conclusive.

sporangia. Isosporous, possibly in some forms heterosporous.[1]

SPHENOPHYLLUM PLURIFOLIATUM. Williamson and Scott, *Phil. Trans.*, vol. clxxxv., p. 920, pls. lxxv., lxxxiii., 1894.

*Asterophyllites Sphenophylloides.* Will. *Phil. Trans.*, vol. i., p. 41, pls. i.-iv., 1874. [Type specimens from the Coal-Measures of Oldham in the Williamson Collection, British Museum.]

Many linear leaves in each whorl (18 to 24?). Surface of young stems marked by three longitudinal grooves. Medullary rays in the form of groups of parenchymatus cells in the spaces between the truncated angles of the secondary tracheæ; the groups connected laterally by means of radially elongated cells. Continuous rows of medullary ray cells rare. Deep seated periderm.

S. INSIGNE. (Williamson). *Phil. Trans.*, vol. clxiv., p. 41, 1874, and Williamson and Scott. *Phil. Trans.*, vol. clxxxv., p. 926, pls. lxxvi., lxxxiii., lxxxiv., lxxxv., 1894.

*Asterophyllites insignis.* Williamson. Mem and Proc., *Manchester Lit. and Phil. Society*, vol. iv. [4], p. 13, 1891. [Type specimens from the Carboniferous beds of Burntisland; in the Williamson collection, British Museum.]

Leaves probably not more than six in each whorl. Cortex grooved in young stems. Tracheæ of primary xylem smaller in diameter than in *S. plurifoliatium.*

*S. plurifoliatum.*—Longitudinal canal at each angle of the primary xylem strand; spiral tracheæ more numerous than in the preceding species. Outer cortex of thinner walled cells than in *S. plurifoliatum.* Tracheæ of secondary xylem with scalariform markings on radial walls. Medullary rays of regular rows, of one to two cells in breadth, extending through the entire thickness of the xylem. Phloem contains wide sieve- tube-like elements. Deep-seated periderm.

In describing the fructification of *Sphenophyllum*, Wil-

[1] Kidston, in his definition of *Sphenophyllum*, speaks of it as heterosporous. The heterosporous example described by Renault is, however, extremely doubtful, and as yet we have no actual proof of the heterospory of this genus (see Kidston, p. 58; also Williamson and Scott, p. 942).

liamson and Scott adopt the generic name of *Sphenophyllum*, while Count Solms prefers Binney's term *Bowmanites*. The question of terminology in palæobotany is often a difficult one. When we have very definite evidence that a cone belongs to a certain genus, it would appear the obvious course to speak of both under the same generic name. On the other hand there is something to be said in favour of retaining a special term for detached strobili, which cannot be certainly referred to their respective vegetative stems. In the case of *Sphenophyllum Dawsoni* (Will.) it may be, as suggested by Zeiller, the strobilus of *S. cuneifolium* (Sternb.); as our knowledge increases, detached cones must frequently be referred to certain specific forms of stems, and the confusion would probably be lessened if a distinct generic name were in the first instance assigned to isolated cones. The use of distinctive names for the fructification of genera has been found convenient in the case of *Lepidodendron*, *Sigillaria*, and *Calamites* (*Lepidostrobus*, *Sigillariostrobus*, and *Calamostachys*). Such names suggest the strobili of the different genera, and in looking through a list of species one recognises at a glance those which stand for reproductive structures. Solms does not adopt the generic designation of the fructification of *Sphenophyllum* corresponding to *Calamostachys*, as he considers such a term as *Sphenophyllostachys* too long and inconvenient. In coining new names sesquipedalian words should, as a rule, be avoided, but in discarding the genus *Sphenophyllostachys* one is departing from a recognised and convenient custom for a reason which hardly seems adequate. *Bowmanites* is the older name, but now that its true position is known, it should be replaced by a term which expresses the fact of its connection with *Sphenophyllum*. I would suggest, therefore, that the name *Sphenophyllostachys* be adopted for the strobili of *Sphenophyllum*.

SPHENOPHYLLOSTACHYS DAWSONI (Will.). (*Mem. Manchester Lit. and Phil. Soc.*, vol v., p. 28, pls. 1-3, 1871.)

VOLKMANNIA DAWSONI. *Ibid.*

BOWMANITES DAWSONI. Weiss. *Steinkohlen Calamarien*, ii., p. 200, 1884.

Slender axis bearing alternating whorls of bracts (14 to 20), cohering basably and free distally as long linear segments extending upwards through about six internodes. A single verticil of long and slender sporangiophores on each whorl of bracts. The sporangiophores bend inwards at the apex and bear single sporangia. Isosporous. Spores with spinus outgrowths. Probably the strobilus of *Sphenophyllum cuneifolium* (Sternb).

SPHENOPHYLLOSTACHYS RÖMERI (Solms-Laubach). *Jahrb. Geol. Reichs. Wien*, Bd. 45, Heft. 2, p. 225, Pls. ix. and x., 1895.

Axis and whorls of bracts similar to those of *S. Dawsoni*, except that in each verticil of bracts the free linear segments extend nearer to the strobilus axis. More than one whorl of sporangiophores on each whorl of bracts, probably three. Each sporangiophore forked distally, and bearing a sporangium on the inwardly bent tip of each diverging branch. Isosporous. Spores similar to those of *S. Dawsoni*.

Genus TRIZYGIA. Royle. *Botany and Nat. Hist. Himalayan Mts.*, p. 431, 1834.

This generic name was proposed in 1834 for a genus of plants occurring in the Glossopteris flora of India.[1] Little is known as to its real affinities or structure, but Zeiller[2] has recently pointed out the doubtful generic value of its characters, and he regards it as most probably a form of *Sphenophyllum*. The slender stem bears verticils of wedge-shaped leaves in three pairs at each node, the anterior pair being smaller than the two lateral pairs.

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[1] For figures see Feistmantel, pls. xi. A. and xii. A. [2] Zeiller (2).

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A. C. SEWARD.

# THE WORK OF THE PORTUGUESE GEOLOGICAL SURVEY.

THE geology of the Spanish Peninsula is imperfectly understood; but it is not without a special interest of its own. It is here, if anywhere in Europe, that we should expect to find among the more ancient faunas some indications of a warmer temperature than prevailed farther north, or of some other difference due to difference of latitude. It is the only part of Southern Europe where there is a really extensive development of the Lower Palæozoic rocks; but unfortunately these are still almost unknown.

Of late years the re-organisation of the geological surveys of Spain and Portugal has led to a great increase in our knowledge of those countries, and the recent appearance of a new part of the *Communicações da Direcção dos Trabalhos geologicos de Portugal* affords a good opportunity of recapitulating what has been accomplished in that country.

It is impossible to look at this and the other publications of the Portuguese Survey without a word of praise for the beauty of the plates with which they are illustrated, and the admirable way in which they are printed. It is a painful reflection to an English geologist that the inimitable work of our own Geological Survey should be presented to the world in a style so far inferior; and that the enlightened Government of a great empire should in this respect be so far behind that of a small and not very wealthy country like Portugal.

We may, however, be allowed to express our regret that so few of the memoirs of the Portuguese Survey are accompanied by maps, for without a map it is extremely difficult to follow the text of a stratigraphical paper; and without making a map it is, or should be, almost impossible for the worker himself to be sure that his views are correct. We regret too the long delay in the publication of a new edition of the general geological map of Portugal. Delgado is twenty years old, and although later information is elsewhere available, it is surely time that the Geological Survey

should take upon itself the production of a map more in accordance with modern needs.

By far the greater part of Portugal is occupied by ancient rocks of Archæan and Palæozoic age, and by eruptive masses which probably belong to various periods. All the higher mountains are formed of such rocks; and it is only in the plain of the Tagus and along the coasts that any later beds are to be found. The most extensive area of Mesozoic rocks forms a broken triangle with its base parallel to the Tagus between Lisbon and Torres Novas, and its apex at Oporto. Mesozoic rocks also occupy a narrow strip of country along the southern shores of Portugal in the province of Algarve. They are found too in the Serra da Arrabida, which forms the prominent cape south of Lisbon; and at São Thiago de Cacem and Cabo de Sines farther south. The largest area of Tertiary deposits is that which forms the plain watered by the Tagus and its tributaries.

*"Azoic" Rocks.*—The so called Azoic rocks, in which no fossils have hitherto been discovered and which are presumed to be older than the Cambrian, are best developed east of the Tertiary basin of the Tagus in the province of Alemtejo. But it cannot be said that their age has been determined with certainty, and the supposed absence of of fossils may be due to imperfect examination.

*Lower Palæozoic.*—In spite of the extensive area occupied by schists and other rocks of supposed Palæozoic age undoubted Cambrian fossils have been found at only a single locality in Portugal. So long ago as 1876, between the "Azoic" rocks of Alemtejo and the Lower Carboniferous of the borders of Algarve, Delgado recognised a series of beds which he believed to be distinct from both; and in this series, near the mines of San Domingos, were found *Nereites* and other forms which are usually believed to be tracks of animals. They are quite insufficient to determine the age of the beds, and it was chiefly from a lithological resemblance to certain rocks in the North of Portugal that Delgado referred them to the Cambrian (15).

More recently (21), however, trilobites have been discovered near Villa Boim, some 10 km. west of Elvas; and

these trilobites appear to belong to the characteristic Cambrian families *Olenidæ* and *Conocephalidæ*. Delgado, indeed, compares them with the genera *Liostracus* and *Leptoplastus*; but unfortunately no figures have yet been published, and all that the descriptions enable us to say is that they probably belong to the *Olenus* group. This discovery is of great interest, for at one time it was believed that the *Olenus* fauna was absent in Southern Europe. Recently, however, it has been found also in Sardinia.

The Ordovician and Silurian rocks are much better developed than the Cambrian, or at least they have been far longer known and have yielded fossils much more abundantly. One of the best known localities is that of Vallongo, 10 km. E.N.E. of Oporto, where Sharpe obtained a number of Ordovician fossils which were described by himself and others (41). Recently, Delgado has published a new list of the forms from this neighbourhood, and he recognises three distinct horizons (19). But there is some confusion in the identification either of the horizons or of the fossils; for from the third horizon he records, for example, both the Lower Ordovician form *Acidaspis Buchi* and the Silurian species, *Phacops Downingiæ*. Most of the Vallongo specimens are certainly Ordovician, and among them are *Placoparia*, *Calymene*, *Tristani* and others, characteristic of the Angers slates of France.

At Bussaco, some 20 km. north of Coimbra, Silurian fossils, as well as Ordovician, are found in some abundance. The Ordovician beds consist of a lower division of quartzites, sandstones, black shales and limestones, with *Calymene Tristani*, *Placoparia Zippei*, etc.; and these are succeeded by ochreous argillaceous rocks with *Phacops Dujardini*, etc. The Silurian is represented by blue shales and argillaceous schists with graptolites, *Cardiola interrupta*, and the thin-shelled Orthoceratites which Forbes called *Creseis* (38). Such forms are characteristic of our Lower Ludlow, and to a certain extent of our Wenlock beds.

South of the Tagus, in the neighbourhood of the granitic mass of Portalegre, Delgado has reported the presence of Ordovician beds (15). They commence with a series of

quartzites containing numerous "bilobites," similar to those which in the North of Portugal, and in parts of France, are found at the base of the Ordovician system. It is unnecessary here to enter into the controversy concerning these forms. Nathorst has given strong reasons for believing them to be the tracks of animals; but Delgado strongly opposes this view and maintains them to be algæ (16).

In the neighbourhood of Portalegre there is found also a small patch of schists containing *Monograptus* and some casts of bivalves (15). The relations of these to the surrounding beds are unknown; but if the graptolites are correctly referred to the genus *Monograptus*, they must certainly belong to the Silurian.

So far then as they are known, the Lower Palæozoic rocks of Portugal do not favour very strongly the view that there was any very marked difference in Older Palæozoic times between the faunas of Northern and Southern Europe. Nevertheless, *Placoparia*, *Calymene Tristani*, and *Acidaspis Buchi*, which are characteristic fossils in France and the Spanish Peninsula, are by no means common in Britain, although they have been found there. In short, we have no sufficient data as yet to show how far the fauna of Southern Europe resembled or differed from that of the North.

*Upper Palæozoic.*—There is only one locality in the whole of Portugal where the Devonian has yet been recognised, and this is near the Ordovician quartzites of Portalegre. A band of schists was discovered by Delgado containing *Phacops latifrons*, *Cryphæus*, and broad-winged Spirifers (15).

The Lower Carboniferous on the other hand occupies a wide area in the South of Portugal, where it forms the greater part of the hilly region on the northern borders of the province of Algarve. Like all the other Palæozoic rocks of Portugal, they have never been studied in detail, but they consist of schists and grauwackés, without either quartzites or limestones, and they contain *Posidonomya* (like *Becheri* and *Pargai*), and *Goniatites* (*cf. crenistria*) (15). Hence it appears that the Lower Carboniferous

belongs to the "Culm" facies so widely developed in Central Europe.

The Upper Carboniferous is very restricted in extent, and its distribution bears no relation whatever to that of the Lower Carboniferous. From the character of the deposits, and the mode of their occurrence, there can be little doubt that the Upper Carboniferous of Portugal was laid down in comparatively small basins not unlike those of the Central Plateau of France. It invariably rests unconformably upon very much older beds, and consists very largely of coarse conglomerates.

The most extensive area is met with in the North of Portugal, where the coal measures form a band stretching from the sea-coast at Espozendo (North of Oporto) in a S.S.E. direction across the Douro as far as Pijao in the province of Beira (22). The coal of this band near Vallongo was taken by Sharpe to be of Silurian age (41).

Farther north there is another band some 22 km. long and 700 m. wide in the neighbourhood of Bussaco (38, 26); and lastly, South of the Tagus there is a very small patch at Moinho d'Ordem near Alcacer do Sal (28).

The fossil plants from these three basins have been described by several writers, and according to Gomes (22) they indicate that the deposits of all three are of the same age, *viz.*, that of our Coal Measures. But Wenceslau de Lima has recently revised the flora of Bussaco, and from various considerations, and especially from the presence of *Walchia* and *Callipteris*, he has been led to conclude that the coal bearing deposits of Bussaco belong to the Rothliegende, or to the passage beds between Carboniferous and Permian (26). He believes, however, that the coal of the other basins is of somewhat earlier date. It is remarkable that in the Bussaco beds a crustacean has been found which W. de Lima refers to the genus *Eurypterus* (27).

*Trias.*—The Palæozoic rocks of Portugal are uncomformably overlain by a series of red and white sandstones and conglomerates, to which Choffat has given the name of

"grès de Silves". North of the Tagus these sandstones form the eastern border of the Mesozoic area, stretching in a narrow band from Aveiro nearly to the town of Thomar. South of the Tagus the "grès de Silves" is met with at São Thiago de Cacem, at Carrapateira (N. of Cape St. Vincent), and again as a narrow band resting upon the Palæozoic rocks in the littoral region of the province of Algarve (9).

North of the Tagus the sandstones begin to alternate in the upper part of the series with dolomitic and argillaceous limestones, and these in turn are surmounted by dolomites without sandstone belonging to the Sinemurien (40). In the lower or sandy part of the series there are several beds which contain remains of plants. These have been examined by both Heer (23) and Saporta (40) and seem to indicate a Rhaetic or Infraliassic age.

The calcareous beds which occur higher up in the series are called the "beds of Pereiros". They often contain marine fossils, for the most part gastropods and lamellibranchs, which are believed by Choffat to belong to the Infralias.

In Algarve the general succession is very similar. The red sandstones of the lower part of the series have yielded no fossils. The dolomites of the upper part contain marine forms; and as in the north, the dolomites gradually increase at the expense of the sandstones. They are overlaid by marls, spotted with white, which often contain gypsum, but no fossils (9).

*Jurassic.*—Jurassic rocks are found in four separate areas, all of which are in the neighbourhood of the coast-line. They extend, with some interruptions, from Aveiro in the north of Portugal, to Cintra. They occur also in the Serra da Arrabida, which forms the promontory south of the Tagus which we call Cape Espichel. They are found in the next important Cape to the south, the Cabo de Sines; and lastly, they are extensively developed along the coast of the province of Algarve.

The system is divided by the Portuguese geologists into three stages, corresponding with the three divisions adopted in Central Europe and named in ascending order,

the Lias, Dogger, and Malm. There is, unfortunately, no general account of the whole; but Choffat has given us a brief description of the Jurassic of Algarve (9), and a fuller account of the Lias and Dogger (2) and of the lower part of the Malm (12) north of Lisbon.

The Lias and Dogger are almost entirely marine, and correspond very closely with the contemporaneous beds of Central Europe. Many of the northern zones have been recognised in Portugal and further research no doubt will reveal others. It may be noted here that Choffat includes the Callovian in the Dogger (5, 8).

The Malm, on the other hand, is a much less purely marine formation, and in places contains beds of lignite, which are sometimes, for example, at Cape Mondego, extensive enough to be worked. It differs considerably from that of Northern Europe, and is very variable in character. Everywhere, however, it may be divided into two stages, the Lusitanian below and the Neo-jurassic above.

North of the Tagus the Lusitanian as it exists in the country of Torres Vedras has been described by Choffat (12). This area includes the chain of Montejunto and the low-lying country limited on the east by the Tertiary basin of the Tagus and on the south by the Cretaceous rocks of the neighbourhood of Lisbon. The Lusitanian beds rest upon Callovian deposits with *Peltocerus athleta* and *Cosmocerus calloviense;* and they themselves consist chiefly of limestones in the lower part and of clays with banks of sandstone in the upper. The limestones contain various Ammonites, among them several Oxfordian forms; while in the clays are found numerous gastropods and lamellibranchs, which are mostly similar to those from the Sequanian or Astartian of the Jura.

Elsewhere, however, excepting in the eastern part of Algarve, the Oxfordian fauna has not been discovered, and the Sequanian rests directly upon the Callovian, but without any visible unconformity.

In the Montejunto country the passage between the Lusitanian and the succeeding Neo-jurassic beds is formed

by clays with *Lima alternicosta.* Sandstones alternate with these clays and in the succeeding beds become predominant. They extend up to, and include the representatives of the Portlandian.

South of the Tagus nearly the whole of the Malm, and indeed nearly the whole of the Jurassic, consists of limestones, dolomites and marly limestones, excepting in the uppermost part, where coarse conglomerates are sometimes found (9). But the various localities show very different developments. In the eastern part of Algarve the series is complete, but towards the west there are very considerable gaps in the succession. In Western Algarve and at Carrapateira the lower part of the Lusitanian is absent. During the Jurassic period, therefore, Eastern Algarve lay under moderately deep water throughout, while the rest of Portugal, with its shallow water and lignite deposits and its interrupted sequences, seems during the deposition of the Malm to have been in great part land and in part covered only by shallow water. Portugal in fact is an exception to the general rule which obtains through most of Europe, that the close of the Lias period was marked by a great extension of the ocean. Throughout Portugal the Lias is entirely marine; while the Malm is generally in part absent and in part shows numerous brackish and lacustrine deposits (40).

*Cretaceous.*—The Cretaceous deposits occupy even a smaller space than the Jurassic. They cover, however, a good deal of ground immediately north of Lisbon; and several patches are found within the Mesozoic area farther north; while Cretaceous beds are also known in the littoral region of Algarve.

Again M. Choffat is our chief authority. He has described in some detail the Cretaceous of Cintra, Bellas, and Lisbon (extending from Cabo da Roca to the Tagus) (4); the Cretaceous patches of Torres Vedras, Peniche, and Cercal farther north (11); and also the Cretaceous band in Algarve (9). As in the Cretaceous of Southern Europe generally, Rudistae are among the characteristic fossils, although the genus *Hippurites* itself appears to be absent.

Choffat divides the system into the following subdivisions (in ascending order) :—

Infravalanginien.
Valanginien.
Hauterivien.
Urgonien.
Beds of Almargem.
Bellasien.
Cenomanian Limestone.

Most of these subdivisions are the same as those adopted in Southern France and bear the same names. The beds of Almargem represent the Aptian, and possibly the Albian, of northern geologists, while the Bellasien is in part, and perhaps entirely of Cenomanian age. The "Cenomanian Limestone" may possibly include the lowest part of the Turonian ; but no higher beds of the Cretaceous are known.

The difficulties which have been encountered in the examination and correlation of the Cretaceous deposits of Portugal, were due to the rapid changes in lithological character which the beds undergo even in a very short distance. The result of this has been that different facies of the same horizon have often been described as different horizons.

These rapid lateral changes were due without doubt to the nearness of the Cretaceous coast-line. Even in the Upper Jurassic, as we have already seen, transported materials play a considerable part, and this is still more true of the Cretaceous. Coarse sandstones and conglomerates are here abundant.

There are only two districts, namely, the region of Cintra and Bellas and that of Eastern Algarve, where the base of the Cretaceous is represented by marine beds, and both these districts are near the present sea margin. Here the whole of the Cretaceous excepting the beds of Almargem consists of marly limestones with a marine fauna.

In the Cintra area, for example, the beds from the Infravalanginien to the Urgonien, and the Bellasien also, consist chiefly of marls and marly limestones. The beds of

Almargem consist of sandstones at the base and summit and limestones in the middle, the respective development of the sandstones and limestones varying greatly. The Cenomanian Limestone consists of compact limestones with some marly limestones and sometimes with beds of flint. They contain *Rudistes Sphaerulites*, etc.

In the country of Bellas, which lies only 5 km. farther east, sandstones are much more developed, and form not only the Aptian but also the whole of the Valanginian. Twenty kilometres north east of Bellas the sandstones invade all that lies below the Bellasien. This is the case also at Torres Vedras and Cercal; and farther north still, sandstones form nearly the whole of the Cretaceous.

As far north as Torres Vedras there is no gap between the Jurassic and Cretaceous; but beyond this point the base of the Cretaceous is absent, and the gap becomes greater as we proceed farther north.

In general the coarseness of the material diminishes towards the west; and from this and the other facts noticed we may conclude that in the region north of the Tagus, during the Cretaceous period land lay towards the north and east, and gradually sank, the sea attaining its greatest extension in Cenomanian times.

In the extreme east of Algarve the lower part of the Cretaceous (the Neocomian of many authors) is entirely marine. At S. João-da-Venda it is replaced by sandstones and conglomerates; while in Western Algarve it is entirely absent. Here again then, as in the Jurassic period, we find that land lay in Western Algarve during a considerable part of the time while the eastern part of the province was under the sea.

Since both the Cretaceous and Upper Jurassic of Portugal consist largely of shore deposits, it is not astonishing that numerous beds of lignite have been discovered in them, and that plants are very abundant. These have been described in detail by Saporta, who finds a gradual passage from the Jurassic to the Cretaceous flora without any sudden change such as is met with in other parts of Europe (40).

*Tertiary.*—Towards the close of the Cretaceous period there appear to have been considerable outbursts of basalt, which at their base are interstratified with marls containing a terrestrial fauna. They may, perhaps, be connected with the great outflows of North-western Europe; but they have not yet been properly examined, and up to the present their age remains uncertain.

The Tertiary beds which occupy so much of the plain of the Tagus and occur elsewhere in smaller patches have not attracted much attention from the Portuguese geologists, and little seems to have been written concerning them.

The more recent deposits, however, have been the subject of various papers (13, 33, 39, 43), and evidence has been brought forward showing the existence of prehistoric man (1, 10, 17, 34-37), and also, in the higher mountains, of the previous presence of glaciers (20, 44). Striated blocks and other evidences of glacial action have been discovered in the Serra d'Estrella.

"*Tiphonic*" *valleys.*—But perhaps the most interesting problems in the whole of Portuguese geology are those which concern the valleys called by Choffat "tiphonic" (3, 6). These are met with chiefly in the Mesozoic area North of the Tagus. In all cases there is a level floor bounded on several sides by hills of Jurassic rocks. The floor is formed of reddish marls, the "Marls of Dagorda," and sometimes upon it rise low ridges of dolomite. Choffat has shown that the dolomites contain fossils of the same age as the Beds of Pereiros (Infralias), and he believes the marls to belong to the same period. The marls *appear* to dip below the Jurassic beds which form the hills surrounding the valleys; but the lower beds of the Jurassic are always absent. The appearance of a conformable sequence is in fact merely deceptive and the valleys are bounded by faults, the floor of the valley being raised so that the Infralias of the floor abuts against the higher Jurassic beds of the surrounding hills.

Connected with these tiphonic areas are considerable outbursts of ophite and teschenite (3, 6, 30, 31), and Choffat has brought forward some evidence to show that

the eruptions and the formation of the valleys took place in the Tertiary period.

*Eruptive Masses.*—Mention has already been made of the basaltic eruptions which took place at the close of the Cretaceous period, and of the outbursts of ophite which are probably of somewhat later date. But it is probable, too, that some of the great masses of eruptive granite which are so prominent on the geological map of Portugal are at least as recent as these. Ribeiro had long maintained that the granite of Cintra was of Tertiary age, but he never published his evidence. Choffat has shown that this granite has sent out veins into the base of the Malm; and as the higher beds of the Malm and the whole of the Cretaceous, up to the top of the Cenomanian, follow upon these without any sign of disturbance, he holds that the granite cannot have been intruded until after the deposition of the whole series. It cannot therefore be older than Upper Cretaceous (7).

One of the most interesting of the eruptive masses of Portugal is that which forms the greater part of the Serra de Monchique. This consists chiefly of elæolite syenite, which seems to have forced its way into the Culm of this region without ever reaching the surface. The gradual cooling of this mass under great pressure has led to the formation of some interesting varieties of dyke rocks which have been described by various writers, and subsequent denudation has exposed the resulting igneous complex (24 and 25).

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PHILIP LAKE.

# IODINE IN THE ANIMAL ORGANISM.

THE disease called Myxœdema is due to morbid conditions of the thyroid gland in which it no longer exercises its normal *rôle* in the metabolic cycle. It is now a matter of common knowledge that injection of extracts from the thyroids of other animals cures the disease, by replacing the lost internal secretion of the diseased or absent gland. This very remarkable practical outcome of physiological research has prompted several investigators to attempt the discovery of the active chemical substance secreted by the thyroid by which that organ normally influences the nutrition of the nervous system, and to which thyroid extracts owe their efficacy. Though this search can hardly yet be said to have completely attained its object many interesting facts have come to light during its progress, and by no means the least of these is the discovery that there are certain substances in the thyroid which contain iodine in organic combination; as an integral part of living animal structures this element was previously not known to exist.

The work was carried out in Baumann's laboratory at Freiburg, and began with an investigation by Ernst Roos[1] on the influence of the thyroid gland on metabolism. In Roos' paper the history of the subject, so far as it relates to the use of thyroid extracts in the relief of myxœdematous conditions is fully given. The experimental part of the paper describes observations on human beings during treatment, who showed a marked increase in the amount of phosphoric acid which they excreted. The majority of the experiments were, however, performed on dogs; the thyroid had been previously removed, and the gland was administered as food to them. The excreta were analysed and compared with those in healthy dogs. In the latter there is an increase in the excretion of nitrogen (more than can be explained by the nitrogen in the gland given) of sodium

[1] *Zeitsch. f. physiol. Chem.*, xxi., p. 19, 1895.

chloride and phosphoric acid. The rise in sodium chloride does not last as long as the others.

In dogs without a thyroid, the increased excretion of nitrogen and chlorine is even more marked, while that of phosphoric acid is not so great as in healthy animals. There is also a diuretic action.

Some attempts were made to separate and identify the active substance of the thyroid, but this part of the research was incomplete. So far as the experiments went they showed that the material is very stable; this was previously known because good effects follow thyroid feeding; the substance is therefore one which is not altered to any extent by digestive processes. The substance is probably proteid-like in nature, though not an enzyme as Notkin[1] considers.

This conclusion fits in very well with some previous work done by Gourlay.[2] Gourlay separated a nucleo-proteid and an albumin from the thyroid; he further discovered that the colloid matter in the thyroid vesicles is nucleo-proteid and if this passes into the lymphatics, as some have described, he thought it possible that the nucleo-proteid was the active agent. This contains nuclein, a substance not affected by gastric digestion.

Later Frankel[3] separated from the thyroid a crystalline material which he named thyreo-antitoxin; this has the formula $C_6H_{11}N_3O_5$; the evidence that this is really what its name signifies cannot, however, be described as satisfactory.

E Baumann[4] continued the search, and it was here that he came across the substance he called thyro-iodin, which is remarkable among animal products in containing iodine. The glands were boiled for days with ten per cent. sulphuric acid; the liquid after cooling deposited a glocculent precipitate, which after extraction with alcohol is the substance in question. It contains 9·3 per cent. of iodine, and it may

[1] *Wiener med. Wochensch.*, No. 19 and 20, 1895. Notkin named the enzyme he supposed to be the active agent—thyreo-proteid.

[2] *Journ. of Physiol.* xvi., p. 23. [3] *Wiener med. Blätter*, No. 48, 1895.

[4] *Zeit. physiol. Chem.*, xxi., p. 319, 1895.

be a derivation of nucleic acid as it contains 0·54 per cent. of phosphorus, but it was not obtainable from the thymus gland,[1] nor from pure nucleic acid.

From the first Baumann was inclined to the belief that here was the active substance he was looking for; this opinion was, however, expressed with considerable reserve until he had thoroughly tested the hypothesis by experiment; and in this part of the investigation he worked with Roos.[2]

They separated the substance by the use of either sulphuric or hydrochloric (10 per cent.) acid. It is insoluble in these reagents, the other constituents of the gland being soluble. It is thus extremely stable, and is moreover not altered by heat.

In the gland itself, thyro-iodin is, however, chiefly in combination with the proteids. They found that the proteids of the gland which can be dissolved out by saline solution are an albumin and a globulin.[3] It is with the albumin[4] that the greater part of the thyro-iodin is combined; the globulin contains a small quantity, and a third portion is free, that is, not combined with proteid matter at all.

The following table gives some idea of the quantity of iodine in the glands in human individuals.

| | | Dry weight of Organ. | Iodine in 1 gr. of Organ. |
|---|---|---|---|
| Adult human thyroids—Average of 26 cases. | Freiburg | 8·2 gr. | 0·33 |
| " " " 30 " | Hamburg | 4·6 | 0·83 |
| " " " 11 " | Berlin | 7·4 | 0·9 |
| Children's thyroids " 39 " | | | 0 or traces |

[1] Later (Baumann, *ibid.*, xxii., p. 1, 1896) small quantities were obtained from calf's thymus.

[2] *Zeit. physiol. Chem.*, xxi. 481.

[3] For other work in the proteids of the thyroid see Gourlay, *loc. cit.*; Notkin, *loc. cit.*; Bubnow, *Zeit. physiol. Chem.*, viii., p. 1. The last observer found three proteids, one a globulin.

[4] Query, Nucleo-albumin, W. D. H.

In the sheep's thyroid, the percentage of iodine varies from 0·9 to 1·5 in the dry, and from 0·26 to 0·44 in the fresh organ.

In dog's thyroid little or no iodine is found; but the amount is increased by feeding on dog-biscuit. This fact together with the almost complete absence of iodine in the thyroids of children makes an impartial onlooker rather sceptical concerning thyro-iodin as the essential chemical substance in the internal secretion of the thyroid.

Nevertheless Roos[1] maintains, and supports his contention with numerous and exhaustive clinical records, that this substance acts both in men and animals just like thyroid extracts, or feeding on the gland. The resemblance is seen in its action on the general system, in metabolic processes, and in cases of disease (myxœdema and psoriasis).

Other observers have not been so fortunate. Thus Gottlieb[2] found in dogs after thyroidectomy that the administration of thyro-iodin had no influence in preventing the symptoms (convulsions, etc.) that follow this operation, nor in delaying death.

Auerbach[3] suggests that this is because Gottlieb's preparations were poor ones; they only contained 2·8 per cent. of iodine.

The main practical question is therefore unsettled, and must be left to future investigations to decide.

But as a point of scientific interest, the discovery of iodine in the animal body remains as one of great importance, and is the most startling of scientific discoveries made of recent years in the domain of chemical physiology.

Iodine in the thyroid is, however, not a unique occurrence. Almost simultaneously with Baumann's announcement, Drechsel[4] published a paper in which he showed that iodine occurs in other structures in quite a different part of the animal kingdom.

[1] *Zeit. physiol. Chem.*, xxii. p. 18, 1896.
[2] *Deutsch. med. Wochenschr.* xxii., 15, p. 235.
[3] *Centralbl. f. physiol.*, x., p. 133, 1896.
[4] *Zeit. f. Biol.*, xxxiii., p. 85, 1896.

The substance (*gorgonin*) of the horny skeleton of *Gorgonia cavolinii* contains iodine in organic combination. Gorgonin yields a decomposition with barium hydroxide an amido-acid containing iodine (*iodo-gorgonic acid* $C_4H_8NIO_2$) which is crystalline, and has the composition of a iodo-amido-butyric acid; its constitution is, however, not yet certain.

The living substance of the gorgonia contains no iodine or only the merest traces; it is of proteid nature and on decomposition with hydrochloric acid yields, lysine and probably lysatine.[1]

Gorgonin is also a proteid, it yields on decomposition with hydrochloric acid, leucine, tyrosine, lysine, lysatine (?), iodo-gorgonic acid and ammonia.

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[1] According to Hedin's recent work (*Zeit. physiol. Chem.*, xx., 186; xxi., 155, 297), lysatine is a mixture of lysine and arginine. Arginine ($C_6H_{14}N_4O_2$) is a base originally separated from vegetable tissues by Schulze and Steiger (*ibid.*, xi., 43; *Ber. d. deutsch. Chem. Ges.*, xxiv., 2701), and subsequently found by Hedin to be a constant decomposition product of proteids and albuminoids. It yields urea on treating its silver salt with barium hydroxide.

# PETROLOGY IN AMERICA.

THE material for study offered to the American geologist is rich in many respects, and perhaps in no branch richer than in petrology. The vast tracts of Tertiary lavas along and to the west of the Rocky Mountains, the peculiar igneous rocks on the east side of the great watershed, the varied series of lavas, tuffs, and intrusive masses in the Palæozoic and older formations of the Eastern states, the extensive areas of igneous and other crystalline rocks in the Lake Superior region, the Adirondacks, Canada, etc., all present many points of interest, and much valuable work has already been done in the description and study of these rocks. These results we owe in large part to the United States Geological Survey and that of Canada, and to various state surveys: Minnesota, New York, Arkansas, Texas, etc. Besides this official work, systematic petrographic research has been carried on at several universities and colleges, such as Johns Hopkins, Columbia College, Yale, the University of California, and others. It is proposed in this paper to notice a few of the more interesting contributions from American sources during the last two or three years; but in selecting from so large and varied a literature we are compelled to confine ourselves to certain areas and certain groups of rocks.

Among the older formations much attention has been given to the massive basic igneous rocks so extensively developed in some parts of Canada and the United States. Adams (1) has given us a very complete account of the so-called "anorthosites" or felspar-rocks which constitute the chief bulk of what has been named the Norian formation of Canada. They are undoubtedly plutonic rocks of the gabbro family, characterised, however, by a great preponderance of plagioclase felspar, usually labradorite. Next in importance, though much subordinate, to the felspar are augite and hypersthene, or in the Saguenay district olivine.

An original parallel-structure or banding is found in some of the rocks, and cataclastic structures are frequent. When iron-ore is present, it is always highly titaniferous, and in places there are in the anorthosite masses of ilmenite with only subordinate olivine and plagioclase.

The similar and perhaps coëval rocks of the Lake Superior region have been investigated by Lawson (2) on the Minnesota coast. They are fresh, coarse-textured, massive rocks composed almost wholly of felspar. This has been stated by Winchell to be labradorite, while Irving described it as anorthite. Lawson finds both these varieties in the rocks of different localities. The only other constituent present is a little augite, partly in grains, partly in minute parallel inclusions in the felspar, and the rocks are thus of remarkably simple mineralogical constitution.

Bayley has undertaken a general study of the basic massive rocks of the Lake Superior region (3). The most important instalment of his results deals with the great gabbro mass at the base of the Keweenaw formation in North-eastern Minnesota. This has sometimes been regarded as a "flow," but he suggests that it is either a great intrusion in the lower part of the Keweenaw or an older eroded massive rock, upon which the latter has been deposited. As the rock has the characteristic structure of a plutonic mass, it is indeed difficult to believe that it can have consolidated under superficial conditions. Its constituent minerals are felspar, augite, olivine, and magnetite, and variations in the relative proportions of these give rise to different types, very rich in one or other of these constituents. A point of some theoretical importance is that these constituents seem to be the same in rocks which differ widely in total composition. The felspar is always a basic labradorite; the olivine is a hyalosoderite with 34 per cent. of ferrous oxide; the magnetite is not titaniferous in the normal rock, and the aggregates of titaniferous iron-ore well known in Minnesota belong to rocks differing in various respects from the great mass.

Near the base of the main mass of gabbro, along its northern edge, occur a number of partially banded rocks of

more basic nature, which the author ascribes to processes of differentiation in the gabro-magma during its cooling. These include various peridotites and pyroxenites, or as Bayley names them, to mark their affinities, " non-felspathic gabbros ". There are olivine-pyroxene rocks, varying to pure olivine-rocks; pyroxene-aggregates; pyroxene-magnetite rocks; and rocks containing up to 90 per cent. of titaniferous magnetite. Analyses of this magnetite have yielded from 2 to 16 per cent. of titanic acid. Associated with these various basic modifications of the gabbro are rocks differing from the normal gabbro in possessing the granulitic structure. They differ also to some extent in mineralogical constitution, hypersthene largely taking the place of the olivine, while the augite is more or less replaced by biotite and hornblende.

It is well known that rocks of the gabbro family, closely comparable with those of Canada and of the Lake Superior region, are largely developed in the Adirondacks, in the northern part of the state of New York. A large part of these rocks are "anorthosites," composed mainly of labradorite felspar with only subordinate augite and hornblende and usually some red garnet, perhaps secondary. The rocks are much affected by cataclastic structures. These anorthosites form the heart of the mountain region, while more basic gabbros constitute the smaller outlying intrusions and minor portions of the main ridges. The latter rocks have been described by Kemp, as they occur on the eastern side of the mountains, along the western shore of Lake Champlain (4). The felspar seems as before to be labradorite: the other minerals are augite, hypersthene, titaniferous magnetite, and occasionally olivine. In places there are bodies of iron-ore with 13 to 16 per cent. of titanic acid, and these are regarded as representing an extremely basic phase of the gabbro magma itself.

These iron-ores are described in more detail by the same author in a bulletin dealing with the geology of two townships in Essex County, N.Y. (5). The ores are all essentially of magnetite, but those of Westport township are of little value on account of the considerable amount of

titanic acid which they contain, while those of Moriah are practically free from that substance. The titaniferous ores occur in the gabbro, the non-titaniferous in the gneiss of the district. In Canada, Lawson has pointed out exactly the same distinction between the magnetite, whether disseminated or collected in rich bodies, of the gabbros and anorthosites and of the gneiss, respectively.

Smyth (6) has given an account of the different varieties of gabbro in the north-western part of the Adirondacks. One kind consists mainly of felspar and augite, the latter often converted into compact hornblende, but the relative proportions of these constituents vary greatly. The commonest type is very rich in the ferro-magnesian minerals, but there are rapid transitions into a highly felspathic rock. The felspar is in general labradorite, but in the more felspathic varieties of the rock it is a more acid species, with perhaps some orthoclase. Another type of gabbro is hypersthene-bearing. The most interesting, however, is a rock consisting of dominant felspar, with some augite, etc., and showing cataclastic structures. The felspar ranges from a highly twinned plagioclase to a fibrous microperthite, and from a petrographic point of view the rock shows transitions from a gabbro or anorthosite to an augite-syenite. The latter has $65\frac{1}{2}$ per cent. of silica, and about 5 per cent. each of potash and soda. In contrast with the Minnesota gabbro, we notice here that, as the rock varies, the minerals (at least the felspars) vary with it, indicating clearly that the differentiation has here been effected prior to the crystallisation of those minerals.

Without citing other papers on the gabbros and allied rocks of the North American continent, we may briefly advert to two or three points of general interest brought out in the works summarised above. One question of considerable petrological importance relates to the so-called "reaction-rims" which often surround the crystals of certain minerals. These consist of one or more zones of various silicates, etc., interposed usually between some ferro-magnesian mineral and the plagioclase felspar. The phenomenon is common in other districts of America and

Europe. The name applied to it assumes that the bordering minerals are of secondary origin, due to reactions between the felspar and the ferro-magnesian silicate or iron-ore grain, and several writers have ascribed the effect to dynamic metamorphism ; but on these questions there is by no means a common agreement. In the Canadian anorthosites Adams (1) records a zone of red garnet as frequently occurring between pyroxene or iron-ore grains and felspar. In other cases he notes two zones between olivine and felspar, the first being of a pale rhombic pyroxene, the second of pale green actinolite needles, set perpendicularly and sometimes having parallel interpositions of deep green spinel. He finds no evidence of these rims being a secondary effect due to dynamic causes. In the great gabbro mass in Minnesota, as described by Bayley (3, 7), the olivine is often surrounded by a narrow border of diallage or augite which thus intervenes between it and the felspar. This border seems to be certainly an original growth, for it is sometimes seen to be continuous with a crystal-plate of diallage. Elsewhere there is a fibrous intergrowth of the bordering augite with the contiguous labradorite, and this is found surrounding magnetite as well as olivine. Biotite is also found interposed between magnetite and plagioclase, and this Bayley considers due to a reaction, but his reasoning is scarcely convincing. In the anorthosites of the Adirondacks Kemp states that there are no reaction-rims, although the rocks give evidence of great dynamic disturbance. In the basic gabbros, however, he describes a considerable variety of borders round pyroxene, olivine, and magnetite. Between augite and felspar is interposed a zone of brown hornblende crystals ; between magnetite and felspar a first zone of brown hornblende and a second of garnet. In this latter case there may be additional zones, such as biotite immediately surrounding the magnetite, or clear quartz between the hornblende and the garnet. A curious feature is a parallel or micrographic intergrowth of the garnet with the adjacent felspar. Between olivine and felspar are seen in some cases three successive zones, respectively of granular

hypersthene, quartz, and garnet; or again a zone of brown hornblende followed by one of garnet. While speaking doubtfully of the hornblende and biotite, Kemp considers the garnet certainly secondary on account of its peculiar relation to the felspar. The quartz he regards as the residual silica liberated in the conversion of labradorite to garnet. In view of the great variety of micrographic intergrowths known to occur as original products in various igneous rocks, the secondary origin even of the garnet in this case seems to be by no means conclusively established. The hypothesis of secondary reactions is confronted in some cases by chemical difficulties; thus, it is not easy to see how brown hornblende can be produced by reactions between augite and labradorite, or biotite from magnetite and labradorite. It is, however, true in a general sense, as has often been pointed out, that the rims are usually more or less intermediate in chemical composition between the minerals which they separate. Allowing due weight to this fact, it is still to be observed that the succession of zones is in general what would be expected if the minerals had crystallised out from a molten rock-magma, following the normal order of consolidation of the several minerals as laid down by Rosenbusch. It is also a common feature in undoubted products from igneous fusion that the earliest formed minerals tend to serve as a nucleus for the crystallisation of those that follow, and this is most noticeable in plutonic rocks of basic composition. Further the "celyphite" borders round garnets, and other similar phenomena, afford numerous examples of radiating fibrous arrangement, and linear parallel intergrowth of different minerals, which in many cases are, beyond reasonable doubt, of primary origin. It seems, therefore, that we must require more convincing proof before accepting the view that the "reaction-rims" in these gabbro-rocks are in general, or in any considerable part, really due to secondary reactions between the original constituent minerals.

Another interesting feature in these American gabbros is the occurrence in them of considerable bodies of iron-ore, which have clearly been secreted from the gabbro-magma

itself. Vogt has recognised among iron-ores thus connected with basic igneous rocks two chief types, one characterised by titaniferous iron-oxides, the other by nickeliferous iron-sulphides. Some of the former we have already referred to, but examples of the latter are also known in America, the case of Sudbury in Canada being cited by Vogt. Kemp (8) has recently described an occurrence in Pennsylvania, in which the relations are very clearly exhibited. The ore is associated with a dark basic rock, now consisting mainly of hornblende but probably an amphibolised norite or gabbro, which forms a lenticular mass some 500 yards long. The ore occurs as a marginal modification of this rock, and consists of nickeliferous pyrrhotine and copper pyrites with some iron pyrites and secondary millerite (nickel sulphide). These minerals are associated with a certain amount of hornblende in such a manner as to show that they must have been original constituents of the rock.

The Adirondack gabbros afford some fine examples of contact-metamorphism. The phenomena are most striking at the contact of the gabbros with the crystalline limestones of the district, and some well known-mineral localities fall under this head. As described by Smyth (6), the limestones become more coarsely crystalline as they approach the gabbro, and pass finally into a zone consisting of various lime-silicates and other special minerals, up to one or two feet in width. In one occurrence this zone consists of fibrous white wollastonite and grains of green pyroxene, with some sphene and garnet. At other localities there are several bands parallel to the junction, the one in contact with the intrusive rock being of wollastonite, the next a mixture of felspar, pyroxene, scapolite, sphene, zircon, etc., and then coarsely crystalline calcite with much pyroxene. Again, a layer of scapolite may occur in immediate contact with the gabbro. Another mineral recorded is orthoclase in well formed crystals, while phlogopite mica is generally distributed. The author considers that there must have been actual chemical reactions and interchange of material between the intruded magma and the limestone. The gabbro itself shows some modifications at the contact, the

chief mineralogical feature being the coming in of abundant little grains of sphene, which indeed seems to point to a local enrichment in lime. On the western shores of Lake Champlain, again, Kemp (4) describes the limestones as becoming coarsely crystalline near the gabbro and charged with bunches of various silicates and other minerals: quartz, plagioclase felspar, diopside, hornblende, scapolite, brown mica, pyrrhotite, tourmaline, sphene, etc. Scapolite is characteristic in this connection throughout the region, and is always accompanied by pyroxene and hornblende. Further details are given in a later paper (9). The crystalline limestones are associated with various gneissic and schistose rocks, doubtless also metamorphosed sediments, and in the upper part of the series come serpentinous limestones or ophicalcites. In the best sections, at Port Henry, the bunches or patches of silicate-minerals, etc., enclosed in the limestone range up to masses twenty-five to fifty feet thick. They consist generally of a coarsely crystalline aggregate of plagioclase, quartz, and hornblende, with various other minerals as mentioned above. The serpentine in the ophicalcites, as Merrill had already shown, has been mainly derived from pyroxene, but Kemp finds evidence that garnet has also furnished a part. The rocks also contain lenticular patches composed of pyroxene, hornblende, sphene, and phlogopite. These serpentinous rocks occur in the limestones on the west as well as on the east side of the Adirondacks.

Much attention has been given in recent years to the various igneous rock-types rich in alkalies which occur in the region east of the continental water-shed. A number of interesting examples have been described by Weed and Pirsson from the State of Montana. Two types of phonolitic dyke-rocks were first discovered as boulders, but subsequently traced to their sources in the Bear-Paw Mountains (10). One, styled pseudo-leucite sodalite tinguaite, is of interest especially as containing the pseudomorphs of orthoclase and nepheline after leucite already known in Brazil, Arkansas, etc., while fresh sodalite is also

very abundant. The other, described as quartz-tinguaite porphyry, corresponds very closely with Brögger's grorudite, differing chiefly in the presence of large porphyritic crystals of orthoclase. Besides a variety of intrusive rocks, there occur in the Bear Paw Mountains a series of basalts, probably leucitic, but of these no description has yet appeared.

In the Highwood Mountains (11) in central Montana similar basaltic tuffs and flows occur, while distinct volcanic cores are seen breaking through the level Cretaceous strata, which are further cut by a large number of dykes with a radial disposition. The most interesting feature, however, is the laccolitic intrusion which forms Square Butte, an isolated mountain some three or four miles in diameter, and rising about 2500 feet above the surrounding plateau. The laccolitic character of the mass is clearly demonstrated, and its structure has been laid bare by erosion. It consists of two types of rock, an inner portion of an acid felspathic type surrounded by a zone of a basic augitic one. The former is a sodalite-syenite consisting of sixty-six parts of felspar, twenty-three of hornblende, eight of sodalite, and three of analcime : it has already been described by Lindgren and Melville. The dark type is a syenitic rock, in that its dominant felspar is orthoclase, but it constitutes a peculiar basic variety to which the name shonkinite is given. Augite, containing 1 per cent. of soda, makes up about half the rock, olivine and iron-ore occur in smaller amounts, while apatite, sodalite, nepheline, etc., are accessories. Chemical analyses of the two rocks show a notable difference, the former being richer in silica, alumina and alkalies, the latter in iron-oxides, magnesia, and lime. Examination proves that they are parts of a single intrusive body of rock, though the transition from one to the other is a rapid one. The phenomena seem to show conclusively that the magma was injected in a homogenous condition, and was differentiated in place. That the differentiation was, at least in part, effected prior to crystallisation appears from the difference between the ferro-magnesian minerals of the two rocks.

The same authors have studied certain allied rocks occurring at Yogo Peak in the Little Belt Mountains, also in Central Montana (12). These rocks form parts of a mass two miles long and one mile wide occupying a great fracture in the Palæozoic strata. At the eastern point of the peak occurs a syenite with $61\frac{1}{2}$ per cent. of silica and moderately high percentages of magnesia and lime. It is an augite-syenite, though with subordinate hornblende. Westward it gradually changes character in the sense of becoming more basic, until at the summit it contains as much augite as orthoclase. This type, which has about $54\frac{1}{2}$ per cent. of silica, has been named yogoite. At the western point of the peak the augite predominates over the orthoclase, while biotite and iron-ore also become prominent, and pseudomorphs after olivine are sometimes found. The rock here is a shonkinite similar to that of Square Butte. The silica percentage has fallen to 49, while the amounts of iron-oxides, magnesia and lime have increased considerably as compared with those in the syenite. A still more basic type occurs in irregular masses at the contact. In Yogo Peak then we have an intrusive stock of oval form, which shows a progressive differentiation from east to west along its major axis. It may be remarked that, as the several associated rock-types are composed in general of the same minerals, the augite in particular running through all the varieties, the differentiation seems to have been of a kind which has elsewhere been considered to have been effected concurrently with, and as a consequence of, crystallisation. Another interesting point is the occurrence of shonkinite in association with two distinct types of syenite, the sodalite-syenite of Square Butte and the augite-syenite of Yogo Peak; illustrating the fact that a given rock-type may originate by differentiation in more than one way.

A series of specimens, chiefly from intrusive sheets, in the southern part of the same state have been described by Merrill (13), and some of them show evident affinities with the foregoing rocks. One type, described in several examples under the name augite-porphyrite, compares rather closely in chemical composition with the yogoite of

Weed and Pirsson, and seems to be the porphyritic or "dyke" equivalent of that plutonic rock. A rock with porphyritically developed olivine and augite has a peculiar composition. With 47 per cent. of silica it has as much as 21 per cent. of magnesia, and on the other hand 3 per cent. of alkalies, chiefly potash. Iddings has remarked that this rock falls under the type which he has named absarokite. The same remark applies to others from South Boulder, Antelope, and Cottonwood Creeks, which Merrill has described under the title of lamprophyres. These contain mica as well as olivine and augite, but, as before, no porphyritic felspar, while the analyses show that the felspar of the ground-mass is largely of a potash-bearing species.

In his *Origin of Igneous Rocks* (1892), Iddings drew attention to certain dykes and lava-flows of exceptional character occurring in the Absaroka Range in the Yellowstone Park region. These rocks belong to a late stage in the igneous activity of the district. While showing evident consanguinity with the more usual types with which they are associated, they have chemical and mineralogical peculiarities comparable with those of Merrill's rocks in the country farther north. In a later paper Iddings (14) has given a more complete account of these rocks, which constitute what Brögger styles a "rock-series," that is, a number of types representing like phases of differentiation from what may be regarded as the more normal series of basalts and andesites, with which they are associated. Iddings distinguishes three types, absarokite, shoshonite and banakite. They are usually porphyritic, the phenocrysts being of olivine and augite, with labradorite in the two latter types. The ground-mass is rich in alkali-felspars, and in some varieties contains leucite. In absarokite, which is the most basic type with 46 to 52 per cent. of silica, there is no porphyritic labradorite but only olivine and augite. In shoshonite, with a silica-percentage of 50 to 60, and comparatively rich in alkalies, labradorite figures among the porphyritic elements, and in banakite it predominates. In this type mica partly takes the place of augite in the ground-mass, and the rocks are highly felspathic. Silica ranges

from 51 to 61 per cent., and the alkalies jointly from $8\frac{1}{2}$ to 10 per cent., potash predominating. The most acid banakites carry a certain amount of quartz. We have thus a well-defined series of rocks which may be roughly described as alkali-basalts and alkali-andesites.

Passing southward we have next to notice another area of rocks rich in alkalies. Just as the Geological Survey of Arkansas gave us a valuable account of nepheline-bearing and allied rocks in that State, so the Texas Survey has discovered the existence of an extensive development of such rocks in Southern and Western Texas. Kemp in 1890 recorded a Cretaceous nepheline-basalt from Pilot Knob, near Austin, and since then a number of interesting rocks have been described by Osann. Three years ago he drew attention to two types occurring in Uvalde County in the southern part of the state (15). One, forming dykes, is a fresh rock named melilite-nepheline-basalt, the two minerals being present in about equal quantity. There are large crystals of olivine, and the ground-mass consists of augite, nepheline, melilite, magnetite, and perofskite. The other type, forming hills and buttes, is a nepheline-basanite, in which the porphyritic elements are hornblende, augite and nepheline, with some felspar and olivine. Since part of the felspar is a sanidine, and olivine is rare, the rocks approach phonolite in characters.

More recently the same geologist has described a varied group of rocks, plutonic, intrusive and volcanic, in and around the Apache Mountains in the western (trans-Pecos) portion of Texas (16). An elæolite-syenite occurs at Paisana Pass, and another in the Mount Ord Range, to the south-east of the Apaches. In both låvenite is a constant accessory mineral. The Mount Ord rock passes from a normal elæolite-syenite through a fine-grained porphyritic variety to a marginal phonolitic facies. The change of texture and structure is accompanied by mineralogical changes, the malacolite-like augite, hornblende, and mica giving place to ægirine-augite, ægirine, arfvedsonite, and ænigmatite. In the Saw-Tooth Mountains, in the western part of the district, occurs an augite-hornblende-syenite,

without elæolite, and this too passes at its margin into porphyritic varieties with fine-textured ground-mass. The special interest of these latter is that they are rhomb-porphyries, reproducing in the peculiar habit of their porphyritic felspars and in other respects the characteristics of the well-known rocks of the Christiania district. As in other districts of elæolite-syenites and augite-syenites, there are in the Apache Mountains region numerous dykes of tinguaite and bostonite, and to these two types Osann adds a third, more acid, to which he gives the name paisanite. This contains quartz, and has some resemblance to the grorudite of Brögger, but differs from it in having the soda-amphibole riebeckite instead of ægirine. Moreover this mineral occurs not in crystals but in fibrous and irregular patches in the ground-mass, a common character in the soda-bearing amphiboles of the whole group of rocks. The lava-flows of the district, besides basalt and rhyolite, include a type of phonolite which is distinguished by the name apachite. Its special characters are firstly the abundance of amphiboles in addition to augite and ægirine, and secondly the prevalence of microperthitic intergrowths in the felspars of the ground-mass. The amphibole minerals are a brown variety, apparently between arfvedsonite and barkevicite, and a blue one resembling that which Brögger has described in his grorudite dykes under the name cataforite. The age of these various eruptive rocks in Western Texas has not been determined further than that they are post-Carboniferous.

Before leaving the subject of nepheline or elæolite-syenites, we may note a new Canadian occurrence recently described by Adams (17). It occupies a large area among the Laurentian rocks at Dungannon, Ontario. Nepheline is by far the most abundant constituent, and one variety of the rock is composed almost wholly of this mineral with a little hornblende or mica, thus corresponding with the ijolite of Ramsay and Berghell from Finland. The felspathic constituent of the rock is albite to the exclusion of orthoclase, as in the occurrence at Litchfield, Maine, which Bayley has styled litchfieldite. The other minerals present

are either mica or hornblende in different examples, scapolite, calcite, a titaniferous garnet, and zircon, with sometimes sodalite, the last occurring in places in large masses. In the latter veins of orthoclase occur with the relations of a secondary product (18). The rock contains a variety of hornblende with very low axial angle. This yielded on analysis only 34 per cent. of silica, while the alkalies amounted to $5\frac{1}{2}$ per cent.

Rocks of this class are now known from a number of localities in Canada and the north-eastern United States, including Montreal, where the rocks are proved to be of Silurian age, Litchfield in Maine, Salem and Marblehead in Massachusetts, Beemerville in New Jersey, and Red Hill in New Hampshire. Special interest attaches to the numerous dykes within this large region, which are either known or inferred to stand in genetic relationship with the elæolite syenites. These rocks include on the one hand the highly felspathic type bostonite, and on the other hand the complementary products of differentiation represented by camptonite, monchiquite, ouachitite, fourchite, and other peculiar lamprophyric rocks. Rosenbusch founded his type camptonite on Hawes' rock from Campton Falls in New Hampshire. Other examples have been described by Harrington from Montreal, by Kemp and Marsters from the Forest of Dean in New York State, the Hudson River highlands, Whitehall also in New York, etc. From Andro scoggin County, Maine, Merrill (19) has described rocks which might be termed augite-camptonites, containing that mineral both in phenocrysts and in the ground-mass, as well as hornblende, but showing a tendency towards the ophitic structure which places them in an intermediate position between the lamprophyres and the diabases.

The elæolite-syenite of Beemerville was first recognised as such by Emerson, but Kemp (20) has added considerable information relative to its modifications and its attendant dykes. A porphyritic marginal facies of the main mass is of special interest since Brögger has taken it as the type of his sussexite, the basic end-member of his grorudite-sölvsbergite-tinguaite series. It contains 45 per

cent. of silica and 11 per cent. of soda, and consists of porphyritic crystals of nepheline up to an inch in diameter in a tinguaitic ground-mass rich in nepheline and ægirine. The associated basic dykes in this district are mostly rich in biotite, corresponding with the ouachitite of Arkansas and in some cases with the allied type fourchite.

The dykes of the Lake Champlain district in New York and Vermont were noticed by Kemp and Marsters in 1891, and more recently a Bulletin of the National Geological Survey has given a fuller account by the same writers (21). Besides bostonites and diabases, they describe typical camptonites with hornblende, augite-camptonites, monchiquites, and other rocks which from the absence of olivine are placed under fourchite. Near Danby-borough in Vermont, Marsters (22) has described a variety of camptonite differing from the Campton type in having no porphyritic hornblende. This mineral occurs in idiomorphic brown crystals in the ground-mass, while there are also two generations of augite and rather abundant biotite. From Lake Memphremagog, on the Canadian border of the same state, he has recorded (23) a number of lamprophyre dykes associated with dykes of granite, occasionally taking on the characters of bostonite. The prevalent type here is an augite-camptonite, in which both augite and hornblende occur in two generations. Hornblende-camptonite, monchiquite, and fourchite are represented by single dykes.

These various rocks, as will be noticed, do not contain any mineral of the "felspathoid" group (leucite, nepheline, sodalite, etc.). Kemp (24) has, however, drawn attention to a peculiar dyke occurring at Hamburg, N.J., at some distance from the Beemerville elæolite-syenite, and presenting mineralogically some resemblance to the rather ill-defined group of rocks named teschenites. The rock in question consists nainly of biotite and pyroxene set in an interstitial mass of analcime. It contains spheroidal bodies up to 10 mm. in diameter composed of analcime, apparently pseudomorphs after some vanished mineral. Hussak in 1892 had already taken these as indicating destroyed leucite, and Kemp has subsequently placed this beyond doubt by

finding fresher specimens in which some leucite still remains.

The presence of melilite in certain dykes in the region is another point of interest. Smyth in 1892 described a rock from Manheim, N.Y., under the name peridotite, as consisting of abundant olivine, biotite, magnetite, and perofskite, with considerable quantities of alteration-products. Later study of fresher material has enabled him to identify melilite as a constituent, and the rock becomes an alnöite or melilite-basalt (25). The melilite shows the characteristic "peg-structure," but differs optically from the mineral as usually known in having positive double refraction. An alnöite had already been described by Adams from Ste Anne de Bellevue near Montreal (26). Here the porphyritic elements are large crystals of brown mica (anomite), olivine (with conversion to hæmatite), and augite, and the ground-mass consists of the same minerals with melilite, magnetite, apatite, and perofskite, the rock agreeing very closely with that of Alnö, off the coast of Sweden.

Some other porphyritic dyke-rocks which have been called peridotites might perhaps be placed with more propriety under monchiquite as olivine-bearing lamprophyres. Darton and Kemp (27) have described such a rock from De Witt, near Syracuse, in the same district as the Manheim alnöite. It contains abundant olivine and porphyritic crystals of biotite and subordinate augite in an augitic ground-mass of the monchiquite type. The peridotite of Pike County, Arkansas, seems to be closely similar. Of the monchiquites and the typical fourchites and ouachitites of the last-named state it is not necessary to speak—we have referred to the work of the late J. F. Williams on a former occasion. Here, as elsewhere, these peculiar lamprophyre dykes figure as the satellites of plutonic rocks of the nepheline-syenite family, though some occur at considerable distances from any visible outcrop of the latter rocks. These Arkansas intrusions are assigned to an epoch about the close of the Cretaceous period.

We pass westward in our hasty survey to the Rocky Mountain region. Limitations of space compel us to omit

in the present communication any notice of the varied series of Tertiary effusive rocks, as we have already passed over the equally interesting ancient lavas and tuffs of the eastern states; and we go on to notice an important memoir by Whitman Cross (28) on a widely distributed type of intrusive rock. It was about twenty years ago that Peale, Holmes, and Gilbert investigated the nature of the large lenticular masses of igneous rocks which occur in various parts of the Rocky Mountains of Colorado, and have given rise to numerous isolated groups of mountains farther west, in Utah and Arizona. To intrusions of this form, uplifting the strata above them in the fashion of a dome, Gilbert gave the name laccolite, and the highly symmetrical examples described by him in the Henry Mountains in Utah have remained the type of this kind of occurrence. But little petrographical examination was made at that time of the rocks constituting these laccolites, which were generally referred to as trachytes. In 1887 Cross pointed out that this name was not well chosen, and that trachyte in the strict sense is certainly not a common rock-type in the region. In his recent paper, besides adding considerably to our knowledge of the nature of these laccolitic intrusions and the varieties of form which they take on in different circumstances, he has given a very complete account of the rocks which compose them. The most striking feature of these rocks is their uniformity in chemical, mineralogical, and structural characters throughout a very extensive tract, prolonged northward apparently into the Yellowstone Park and perhaps into Montana. The analyses show variation between certain limits in silica-percentage and in some other particulars with a remarkable constancy in certain other features. The alkalies jointly are always about 6 or 7 per cent., potash and soda being in equal quantities in the rocks of Colorado, while the latter preponderates in those of the plateau groups to the west and in the Yellowstone district. All, or nearly all, the rocks are porphyritic, crystals of plagioclase felspar and ferro-magnesian silicates having been formed prior to the intrusion of the magma. Plagioclase is predominant and,

of the dark silicates, hornblende and biotite, hypersthene and augite, appearing only locally. Quartz also occurs, and in some cases very large crystals of orthoclase. The porphyritic elements have continued to grow after the intrusion, and the large orthoclase crystals belong wholly to this stage of the consolidation: the author dissents from the view that phenocrysts are always to be regarded as representing the first or intratelluric period of crystallisation and the ground-mass the second period. In these rocks the ground-mass is of very constant characters, being essentially a granular aggregate of orthoclase and quartz. The several varieties may be grouped generally as porphyrites, usually hornblende-porphyrite, in some cases quartz-porphyrite.

In conclusion we shall briefly notice some of the petrographical memoirs issued as bulletins from the laboratory of the University of California, which, under the direction of Lawson is producing some very useful work in the far West. The first instalment, by the Professor himself (29), deals with the geology of Carmelo Bay, some distance south of San Francisco. The chief interest, from a petrological point of view, centres in the eruptive rocks to which Lawson has given the name carmeloite. These exhibit variations of characters within certain limits, the silica percentage ranging at least from 52 to 60, so that the rocks hold in some respects a position intermediate between basalt and andesite. They have in some cases porphyritic augite and plagioclase, and the ground-mass varies from a holocrystalline one to one rich in glass. The special feature, however, is the presence of crystals of a mineral for which the name iddingsite is proposed. This substance occurs in crystal forms like those of olivine, with a lamellar structure due to a very perfect cleavage, and with a bronzy lustre on the cleavage faces. Optical examination proves the rhombic symmetry of the mineral, and reveals a brown colour with strong pleochroism. Qualitative chemical tests prove it to be a hydrous non-aluminous silicate of iron, lime, magnesia and soda. The characterisation of carmeloite by reference to this mineral seems to imply that the latter is, as Lawson inclines to believe, an original constituent of the rock, and

not a pseudomorph after olivine. Without prejudging this question, it may be remarked that numerous British basalts and diabases contain a mineral agreeing pretty closely with the description of iddingsite, but undoubtedly derived by alteration from olivine, unchanged relics of which sometimes remain in the heart of the pseudomorph.

Palache (30) has described a rock occurring as a lava-flow of late Cretaceous or probably early Tertiary age in the Contra Costa hills, north of Berkeley. It is an acid lava in which soda preponderates largely over potash, and it is accordingly styled soda-rhyolite. It shows gradations from a porphyritic variety with microcrystalline ground-mass to a pure glass, the bulk of the flow having a microspherulitic structure. One variety consists largely of spheroidal bodies up to three inches in diameter, hollow and containing chalcedony, etc.: these are probably altered large spherulites, and the author's account of them reminds us of those so frequently met with in the ancient rhyolitic lavas of North Wales and other districts.

The same geologist has investigated a blue soda-amphibole which seems to have a wide distribution among the crystalline schists of the Coast Ranges of California (31). The rock in which the mineral was specially studied consists of this mineral and albite. The mineral has the usual crystallographic habit and cleavages of a hornblende. It is strongly pleochroic in blue, violet, and yellowish-brown colours, and its extinction-angle is about 13°. It bears a considerable resemblance to riebeckite, and a chemical analysis places it between that mineral and glaucophane. The author names the new variety crossite, as apparently identical with a blue amphibole described by Whitman Cross in Custer County, Colorado, and one more soda-bearing amphibole is thus added to the number already recognised.

Another new rock-forming mineral has been described by Ransome and Palache (32, 33) from the Tiburon Peninsula, north of San Francisco, where it forms an important constituent of a particular crystalline schist. It has been found also in glaucophane-schists near Berkeley and else-

where, and may prove to have a more extended distribution. It is a rhombic mineral with simple crystallographic habit and two conspicuous cleavages, and it has a blue colour with strong pleochroism, which, however, is lost in ordinary thin slices. The specific gravity is 3·08, and the composition is expressed by the formula $H_4Ca\,Al_2\,Si_2O_{10}$, suggesting a comparison with the manganese mineral carpholite. To the new mineral the name lawsonite is given. It is associated in the rock with margarite, epidote, actinolite, glaucophane, and garnet. The glaucophane is peculiar as having an extinction-angle of 13° to 15°, much higher than in the normal mineral and perhaps indicating an admixture of the actinolite-molecule.

A very instructive memoir by Ransome deals with the geology of Angel Island in San Francisco Bay (34). This island consists mainly of the (probably Cretaceous) San Francisco Sandstone, with intercalated beds of radiolarian chert ; but intruded into these are sills of a rock which is doubtfully placed with fourchite, and also a large dyke of serpentine derived from what has been essentially a diallage-rock. The so-called fourchite is composed mainly of augite, not in crystals but in granules of two sizes, with a matrix consisting of an aggregate of minute prisms of some colourless mineral. The latter is referred to zoisite, and is certainly secondary, possibly derived from felspar. There are varieties of the rock which contain little plagioclase prisms, and in particular a spheroidal variety in which the felspar occurs in skeletons, delicate needles, and brush-like groupings. The typical fourchite of Williams, it should be remarked, contains no felspar. The chief feature of interest, however, is the metamorphism produced by the intrusive masses in the bedded rocks, which are converted near the junction into glaucophane-schists. One of these, where the sandstone borders the main sill of fourchite, consists chiefly of glaucophane, albite, and biotite in varying proportions. In other specimens quartz is the chief constituent. The radiolarian cherts are changed into a rock composed of quartz with needles of glaucophane, actinolite, etc., and in less altered examples blue glaucophane needles have been

developed while the outlines of the radiolaria are still recognisable. Glaucophane-schist has also been produced at the contact of the sandstone with the serpentine. The blue amphibole mineral in these various rocks seems to be in general true glaucophane, though it is possible that crossite or other varieties may also occur. These observations are evidently of importance with reference to the origin of the glaucophane-schists in general of California, and perhaps of other districts. Lawson has expressed a confident opinion that all the glaucophane-, hornblende-, and mica-schists in the Coast Ranges are products of contact-metamorphism by basic eruptive rocks.

Some other points in the geology of Angel Island are of interest in connection with an earlier paper by the same author upon the eruptive rocks of Bonita Point, the northerly horn of San Francisco Bay (35). The strata here still belong to the San Francisco Sandstone, and include beds of "red jasper" corresponding with the radiolarian cherts mentioned above. The chief igneous rock is a basalt with what is for convenience called a spheroidal structure. The distinct portions into which the mass divides are, however, not spheroids but rather bale-like and pillow-like bodies, which give the effect of having been squeezed against one another while in a plastic state, a description which would apply to part of the "fourchite" of Angel Island and to some "spheroidal" basalts and diabases in other regions. The author expresses the opinion that the rock was poured out as a somewhat viscous lava (the viscosity being ascribed to the 2 or 3 per cent. of titanic acid present), successive sluggish out-pourings being piled upon one another, becoming sometimes spheroidal by rolling and sometimes lenticular by flattening. In addition to the basalt, there is at Bonita Point an intrusive diabase containing iddingsite, which Ransome regards as probably a pseudomorph after olivine. This has in places a spheroidal structure of a somewhat different kind attributed to a kind of flow-brecciation. The "fourchite" of Angel Island shows in one intrusion a spheroidal structure allied to brecciation, and in another the peculiar structure described in the basalt. The

author is thus brought to the conclusion that this feature is not restricted to surface-flows, but he believes that rocks exhibiting it must have been erupted under very nearly surface condition. This is to some extent in accordance with the opinion expressed by Fox and Teall with respect to the dolerite associated with the radiolarian chert of Mullion Island, Cornwall. This rock, "separated into rude rolls by curvilinear joints," they suggest may represent a submarine flow injected between the layers of the stratified series near the sea-bed during deposition. The association of these curious spheroidal rocks with radiolarian cherts at a number of localities in California and elsewhere seems significant, but the mechanism of igneous eruptions under a great pressure of water is a subject upon which it would not be safe to speculate at present.

We have already noted Ransome's account of the serpentine of Angel Island, which is demonstrated to be a decomposed diallage-rock. Palache (37) has described the serpentine of the Potrero, San Francisco, which is also of igneous origin and intrusive in the sandstones. In this case the rock has been derived from one consisting of olivine, enstatite, diallage, chromite, and magnetite, and so belonging to the lherzolite type. Lawson finds serpentine rocks in other parts of the district to be in all cases derived from igneous rocks, peridotites or pyroxenites.

The work embodied in the various memoirs which we have cited has gone far towards removing some serious difficulties felt by many readers with reference to the geology of the Pacific Slope as interpreted in Becker's monograph on the Quicksilver Deposits (1888). The San Francisco Sandstone and its associated rocks correspond with the Knoxville group of that writer, who endeavoured to explain the characters of many of these rocks by various obscure processes of metamorphism operating upon sedimentary deposits. The bedded jaspers are now proved to be not silicified shales, but original siliceous deposits, largely due to radiolaria, though Lawson believes much of the silica to have been chemically precipitated. The "pseudodiabases" and "pseudodiorites" of Becker, formerly stated

to be metamorphosed sediments, are all recognised as true igneous rocks, either contemporaneous or intrusive; and the serpentines, instead of arising from the metamorphism of various sedimentary rocks, are found here, as in other countries, to be decomposed basic and ultrabasic eruptives. Of the formidable series of supposed metamorphic rocks described in Monograph XIII. there remain only the glaucophane-bearing and other crystalline schists, and these seem to be in all cases contact-zônes bordering intrusions of basic rocks (36).

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# GOLD EXTRACTION PROCESSES.

FROM very early times the ancients were attracted by the beautiful colour, the brilliant lustre, and the indestructibility of gold, and spared no pains in the endeavour to acquire it. In the code of Menes, who reigned in Egypt some 2000 years before the time of Moses, the ratio of value between gold and silver is mentioned, one part of gold being declared equal in value to two and a half parts of silver, and it is, therefore, clear that the extraction of both metals from the deposits containing them must have been carried on before that time. It is indeed probable that gold was the first metal observed and collected, since it occurs in fragments of all sizes in loose sand, and the operations of collecting the larger pieces and melting them together are so simple. Among the rock carvings of Upper Egypt there are several illustrative of the art of washing auriferous sands by stirring and working them up by the hand in hollowed-out stone basins, and subsequently melting the gold in simple furnaces with the aid of mouth blow-pipes. The earliest of these carvings is supposed to date back to about 2500 B.C. However, in ancient times gold appears to have been mainly derived from India, and that country continued to supply most of the gold used in Europe until the discovery of America by Columbus.

In order to collect alluvial gold, the sands were washed down over smooth sloping rocks by means of running water, and the particles of gold sinking to the bottom of the stream by reason of their high density, were entangled and caught by the hair on raw hides spread on the rocks. Among the hides used were sheepskins, and hence originated the form of the legend of the Golden Fleece. Stripped of its heroic dress this legend of course describes a piratical expedition to win gold which was being obtained from streams with the help of sheepskins by the inhabitants of what is now Armenia. Similar

expeditions have not been unknown in much later times, and the method of obtaining gold by washing river sands is still practised with improvements in matters of detail in many parts of the world. Hides are even now occasionally employed to catch the gold, but sheepswool when used is generally in the form of blankets.

The use of mercury as an aid in the collection of gold contained in river sands or in crushed rock is also of great antiquity. The earliest mention of quicksilver itself appears to occur in the works of Theophrastus, about B.C. 300; but Diodorus of Sicily, who saw gold being extracted from quartz in Upper Egypt in the time of Julius Cæsar does not refer to its use.[1] Only a few years later, however, Vitruvius,[2] about B.C. 13, described the manner in which, by the help of quicksilver, gold was recovered from cloth in which it had been interwoven, and in Pliny's time the separation of gold from its impurities generally by the same means was well known.[3] It is probable that this knowledge was never afterwards entirely lost, although the references to it in the Middle Ages are very scanty. For example Geber[4] in the eighth century was aware that mercury would dissolve considerable quantities of gold and silver, but not earthy materials, and Theophilus the monk,[5] in the eleventh century carefully described the method of washing the sands of the Rhine on wooden tables, the final operation consisting in treating the concentrates with quicksilver for the removal of the gold. Biringuccio[6] was taught the secret of this use of mercury in Italy some time before 1540 in return for the present of a valuable diamond ring, and it is clear that the so-called invention of the amalgamation process in Mexico by the Spaniards in 1557

[1] Diodor., iii., 13. A full translation is given by B. H. Brough in his Cantor lectures on Mine Surveying. *Jour. Soc. Arts*, 1892.

[2] Vit. lib., vii., cap. 8.

[3] Nat. Hist., lib. xxx., cap. vi., sect 32. Quoted in full in Percy's Metallurgy of Silver and Gold, p. 559.

[4] Salmon's *Geber*, cap. 47.

[5] Theophili, lib. iii., cap. 49.

[6] De la Pirotechnia. Venetia, 1540. Lib. ix., cap. xi., fol. 142.

was only the introduction or adaptation of a process already well known in Europe.

The existing methods of washing auriferous sands all depend on two principles, the great density of gold when compared with that of the siliceous material with which it is associated, and, as Baron Born expressed it over 100 years ago, the "elective affinity" of mercury for gold when mixed with impurities. The ease with which gold-amalgam can be collected in spite of its being less dense than gold is of course due to the fact that it is miscible in all proportions with mercury, so that under proper conditions large globules of liquid alloy are formed by the running-together of smaller particles, and the former are readily caught in suitable crevices.

A large number of implements of varied form and efficiency are used in different parts of the world to apply these principles. In operations on a small scale the *batea*, the trough, and the miner's pan are chiefly used. In South America, in West Africa, and in parts of China, the *batea* is used, a wooden vessel having the shape of a very short reversed cone. When held in the hands and filled with gravel and water, a peculiar gyratory motion imparted to it results in the collection of the gold in the apex of the cone, and the light material can then be readily washed away.

A small wooden trough, twelve or fifteen inches long, is used for the same purpose in the far East by the Chinese, the Tonquins, the Annamites, the Malays, and others, the water being made to flow up and down until the gold has settled to the bottom. The miner's pan, a flat-bottomed iron vessel with sloping sides, was first used by the Californian pioneers, but has now become the favourite implement of Europeans for prospecting in all parts of the world. In early days in Australia and California millions of ounces of gold were obtained from the river gravels by its use, but apart from its value in prospecting it is at most a rough and ready means of treating small quantities of rich material, and is only suitable to individual effort.

In combined efforts to treat larger quantities of sand, the machines all consist essentially of a slightly inclined trough, through which a stream of water is made to convey the auriferous sand, mercury being usually sprinkled on at short intervals of time. If the trough is long enough, and the stream of water not too rapid, the gold and mercury sink, and, uniting, are swept along together, until arrested by some inequality of the bottom.

Crevices and "riffles" or obstructions of various kinds are arranged to catch the amalgam. The simplest contrivances are transverse slats of wood nailed to the bottom of the trough or sluice. In Siberia square "pigeon-hole" depressions have been continuously used for more that fifty years. In California, the sluice is paved with square blocks of wood placed an inch or more apart, or with large rounded stones, or ordinary iron rails between which are plenty of crevices where the amalgam can lodge. To catch light spangles of gold, blankets are spread, the loose fibres of which become charged with pyrites and gold, and in New Zealand, plush is a favourite gold catcher.

When a "clean-up" is desired, a stream of clear water is run through the sluice, the riffles are taken up, the mercury and amalgam washed down and allowed to accumulate at some convenient spot, and then ladled out and squeezed in bags of canvas or leather as in the days of Pliny, who describes the process as follows: "ut et ipsum [*i.e.*, argentum vivum] ab auro discedat, in pellis subactas effunditur, per quas sudoris vice defluens purum relinquit aurum". The excess of mercury being thus filtered off, the pasty amalgam, containing about one-third of gold, is retorted.

The methods of conveying the auriferous material to the sluices vary with the scale of the operations and the other conditions. When rocking cradles or the smaller sluices are used, the gravel is shovelled into them. In Siberia, where the valleys are shallow and the inclination of the ground small, the gravel is carried in carts up an inclined plane to an elevated wooden platform whence the sluice starts. In California, where the gulches are deep, the fall

of the ground rapid, and the auriferous deposits of great thickness, the banks of gravel are attacked by jets of water of tremendous power, and the earth washed down and carried through the sluices without being touched by hand, the so-called "hydraulic method". When the gravel beds are below the general level of the country they are raised by the "hydraulic elevator," a jet of water, under a head of as much as 400 or 500 feet, carrying water, sand, and boulders alike up a pipe inclined at some 60° to the horizon, so as to deliver them all at the head of the sluice, the vertical lift being sometimes over 40 feet.

One of the main difficulties in the hydraulic process is in the disposal of the tailings, which are usually discharged into a river or into the sea. The enormous amount of loose sand and gravel, delivered from the hydraulic mines into the Yuba and Feather rivers, California, prior to 1880, filled up their beds to such an extent that in rainy weather disastrous floods ensued, and much valuable agricultural land was buried beneath sterile drift deposits and rendered worthless. The farmers thereupon took action against the mining companies and obtained a perpetual injunction forbidding the discharge of tailings into these rivers. The result has been to stop the use of the hydraulic method in these important districts, and an apparently irreparable blow was inflicted on gold winning industry in California.

In every country as soon as the richest of the placer beds have been worked out, efforts are made to extract the gold from quartz veins. The quartz must of course be crushed, and the crushed material has in the past been generally treated similarly to the auriferous sands occurring naturally. Thus, according to the account given by Diodorus Siculus already referred to, the quartz was reduced to coarse powder by pounding it in stone mortars, then finely ground in handmills resembling the flour mills of the present day, and finally washed down over inclined planks with water, when the lighter material was carried away and the heavy gold retained on the wood. Hollowed-out stone mortars suitable for the first of these operations have been found in many

parts of the world, including Wales, Central America, the Pyrenees, and Transylvania. The stamp mill has no doubt been evolved from the pestle and mortar, but was not used for crushing ores until about the year 1519 when the process of wet stamping and sifting was introduced by Paul Grommestetter in Joachimsthal, the two operations, however, being at first kept distinct.

Agricola has given an exact description of the treatment of auriferous quartz in Germany in 1556,[1] from which it appears that the methods in use at that time were strikingly similar to those still employed in Transylvania and the Tyrol, which were among the districts of which he wrote. Doubtless in these districts the methods have been handed down from generation to generation with little change, while in other countries where they were introduced hundreds of years later the changes have been rapid and striking. In those points in which the older Tyrolean practice differed from the modern one, it resembled the procedure of the old Egyptians. The wooden stamps, shod with hard stone or iron, were arranged in sets of three, and raised by cams to fall by gravity when released. The rock was shovelled dry into the mortar, and coarsely crushed by the blows of the stamps.

Next it was ground as fine as flour in a stone mill supplied with water, and carried by the stream of water into the uppermost of three wooden tubs, whence it overflowed in succession into the other two. Revolving mechanical stirrers furnished with six paddles kept in agitation the contents of the tubs, and "separate even very minute flakes of gold from the crushed ore. These flakes, settling to the bottom, are drawn to itself and cleansed by the quicksilver (lying in the tubs), but the water carries off the dross."[2] Agricola here expounds the theory of amalgamation still adhered to in Austria, where mercury is regarded merely as a useful means of collecting particles of gold which have already been separated from the crushed ore by their great

[1] Agricola. De Re Metallica, p. 233. Basel, 1556.

[2] Agricola, *loc cit.*

density. The Tyrolean bowls still in use at Vöröspatack in Hungary and in a few retired valleys in the Eastern Alps do not differ essentially from the tubs drawn and described by Agricola; and, although wet crushing by the stamps has been introduced, the mortar is not usually furnished with screens.

Elsewhere, the changes in stamp battery amalgamation since Agricola wrote his treatise have been many and great. One of the first was the addition of screens in the side of the mortar, so that the two operations of crushing and sifting were united. In 1767 M. Jars saw these in use in the Hartz,[1] though even then only a single screen of brass wire twelve inches square delivered the product of three stamps, and in several other districts of Germany screens had not been adopted. The most important improvement however has undoubtedly been the introduction of the amalgamated copper plate for catching gold, the complement of the practice of charging mercury with the ore into the battery and so combining the operations of crushing and amalgamation. No mention of either this practice or the use of copper plates appears to have been made before stamp batteries began to work in California in 1850, although they had very likely been used in Georgia for some time previously.

The use of the copper plate was probably suggested by the experience in Mexico and South America of the working of the Cazo process, in which it was well known that amalgam tended to adhere to the copper sides of the vessel unless the proportion of mercury to gold and silver present was less than four to one. Thus Baron Born wrote in 1786[2]: "In new kettles . . . the inside becomes wholly and so perfectly silvered that it never can be cleaned. . . . The silvery coat is daily increased by slow and gradual apposition, and the crusts of amalgama, accumulating on the bottom and sides of the vessels, become gradually so thick that on emptying them they often fall off by their own

[1] *Voyages Métallurgiques*, vol. ii., p. 309. Paris, 1780.

[2] Baron Inigo Born's *New Process of Amalgamation*, p. 122, Translated by Raspe. London, 1791.

weight as silver plates, which, when dry, show the laminated texture of their daily augmentation."

The knowledge of this behaviour of amalgam in the Cazo process must have been common to many who were engaged in exploiting the quartz veins of the West soon after their discovery, and the speedy application of this knowledge is exactly what might be expected from those sturdy pioneers. Nevertheless, the exact date and locality of the introduction of the copper plate remains a matter for conjecture.

The copper plates fixed on the battery in the early fifties were about four inches wide and as long as the mortar, and were placed one on the "feed" side and one on the discharge side just underneath the screens. It was soon found that the plates worked better from the start if mercury was rubbed on them before they were placed in position, and this has now been invariably done for many years. Crushed ore, stones, water, and amalgam are flung violently against the plates and the amalgam is retained in great part.

The scouring action of the pulp on the plates is however always great, and becomes more violent in proportion as the stamps are larger. These are now as much as 1100 and even 1250 lbs. in weight in the Transvaal, an enormous mass when compared with the 750 lb. stamp of a few years ago, and the 120 lb. stamp of the last century. With this increase in weight, it has become desirable to modify the use of the copper plate. The plate on the feed side, long ago condemned by many, is accordingly being discarded more and more, and that below the screens is curved away in such a manner that it cannot be struck directly by the splash from the stamp, while slots in cast-steel plates lining the mortar have been devised for the purpose of catching amalgam.

After leaving the mortar the pulp was treated forty years ago mainly by passing it over inclined tables covered with blankets, much in the same way as Jason may have seen the golden sands worked in Asia Minor. The sands accumulating on the blankets were washed off at intervals

and ground in mills with mercury. In addition the ore was frequently passed over or through baths of mercury (as is still done in many Australian mills) and concentrated in various ways. Amalgamated copper plates over which the pulp flowed were tried, but in Western America were at first almost everywhere rejected,[1] probably owing to the great depth of the stream of ore and water made to flow over them. When this mistake began to be rectified some twenty-five years ago the value of the plates was soon recognised in California.

In the extraction of gold from crushed ore by means of amalgamated copper plates, the pulp is led over their surface in a very thin stream, not more than a quarter of an inch deep. The plates are slightly inclined, wider than the screens from which the pulp issues, and from six to twelve feet long. The pulp does not run down in a regular stream, but in a series of little wavelets which tumble over and over and are supposed to bring every part of the pulp in succession in contact with the amalgamated surface. The catching powers of the plates are thus supposed to be practically independent of the tendency of the particles of gold or amalgam to sink to the bottom of the stream. This theory is not accepted by the Austrian school, and it is certain that native gold is caught more easily in proportion as it contains less silver, so that when the particles of metal consist of an alloy largely consisting of silver, and are therefore of comparatively low density, the yield on the plates is generally poor. In any case, however, the amalgamated plate should theoretically be better adapted for its work than the Tyrolean mill and other machines using mercury baths, owing to the slight depth of the pulp on the plates and the short distance through which the gold particles are compelled to settle before reaching a catching surface. The plates are wiped down with rubber or brushes about once a day and the gold separated in the usual way from the excess amalgam thus collected.

[1] See Nevada and California Processes of Silver and Gold Extraction, p. 61, by G. Küstel, San Francisco, 1863.

Admirable as is the amalgamation process in many respects, it has always been recognised that the extraction of gold by its use is generally far from complete. Besides the comparatively large particles of free gold which are readily saved by amalgamation, all ores contain more or less gold in an excessively fine state of division (the *aurum larvatum* or "disguised" gold of the last century) as well as gold contained in sulphides (*aurum mineralisatum*), and these particles cannot be extracted by the copper plates.

In an investigation on the dimensions of gold particles in ores J. A. Edman[1] observed a single chip of quartz $\frac{1}{50}$ inch in diameter which, when magnified 50 diameters, showed over 300 particles of gold, varying in size from $\frac{1}{1000}$ to $\frac{1}{12000}$ inch not only on the surface of the stone but scattered through the transparent mass. Higher powers showed still greater numbers of smaller particles. The gold contained in pyrites, if, as seems likely, it is generally free, must be often still finer. It has been likened to the mortar in a brick wall, and is almost as difficult to catch as the motes in a sunbeam. Prolonged grinding with mercury no doubt increases the chances of such gold being amalgamated, and hence the success which has frequently attended the use of the Mexican arrastra, where the grinding surfaces are of stone, and of its successor the iron amalgamating pan.

Nevertheless, the yield of gold, mainly owing to the "flouring" and "sickening" of the mercury, is not always good even in these slow-working and therefore costly machines. Mercury, when triturated with ore for a long time, tends to break up into very fine particles which, although apparently clean and bright under the microscope, refuse to run together, and are carried away by the stream of water and lost, together with the gold already taken up by them. Such mercury appears greyish-white, and is said to be "floured". Moreover, when base metals are present in the ore they become amalgamated, and then, oxidising, coat the surfaces

[1] *Mining and Scientific Press*. San Francisco, 12th August, 1892.

of the globules of mercury with black scum, which effectually prevents the amalgam from adhering either to gold or to amalgamated plates. This "sickening" is doubtless also caused by the formation of compounds of the mercury itself. Ores containing sulphides of arsenic or antimony (which are reduced by mercury) are particularly apt to cause "sickening," but manganese dioxide, partly decomposed copper pyrites, zinc blende, and galena are also harmful.

It has been well known ever since the time of Agricola that the gold contained in these minerals, although not easy to extract by mercury, may be readily obtained by concentrating the ore (all finely divided free gold being, of course, lost in the process); the concentrates are then smelted, the gold accumulated in a reduced metal, such as lead or copper, and subsequently separated by cupellation or other means. The impossibility of applying this method of procedure to individual mines far distant from coal beds, and in places where a mixture of different kinds of ore cannot be obtained, prevents the universal application of the method, although in the neighbourhood of such smelting centres as Freiberg and Denver nothing better is required.

The chlorination process, now nearly fifty years old, is of more general value for treating concentrates. Chlorine is a somewhat slow solvent for gold, any particle occurring native which is visible to the unassisted vision requiring many hours for its complete dissolution; but it is well adapted to dissolve the fine flakes existing in pyrites. Unfortunately chlorine has a strongly preferential action on sulphides, and to avoid the enormous waste of gas which a small percentage of these substances cause it is necessary to precede chlorination by careful and complete roasting. After this there is little difficulty in the process. Oxides of the metals, except the alkaline earths, are very slowly attacked by chlorine; and when the alkaline earths are present salt is added in the roasting furnace. Chlorine is applied to the slightly damped ore in the form of gas, or, in more modern practice, as a strong aqueous solution.

After a day or two the liquid is filtered off, and the gold precipitated by ferrous sulphate or sulphuretted hydrogen.

The problem of the extraction of gold contained in pyrites and complex minerals was partially solved by the chlorination process, but the cost of roasting is a stumbling block in many cases, and it was the desirability of avoiding this which led to the introduction of the use of cyanide solutions for leaching, probably the most important event in the history of the metallurgy of gold since the first application of mercury to gold extraction. The extraordinary properties of very dilute solutions of cyanide of potassium were unmarked and in great part unknown until quite recently. That metallic gold is soluble in alkaline cyanides unaided by an electric current remained an interesting but useless fact until it was found that a solution containing only one per cent. of potassium cyanide dissolves gold at least as rapidly as much stronger solutions, although it has a very slow and partial action on most sulphides and other minerals occurring in gold ores. The presence of free oxygen is necessary for the dissolution of the gold, which takes place according to the equation—

$$4\,Au + 8KCy + O_2 + 2\,H_2O = 4KAuCy_2 + 4KHO.$$

The oxygen is supplied from the air entangled in the porous ore, or dissolved in the solutions, in which MacLaurin[1] has shown that it can be retained in the presence of alkaline cyanides in spite of its rapid absorption by the latter with the formation of cyanates. The gold can be recovered by precipitation on zinc and subsequent melting, or by electro-deposition on suitable cathodes, of which lead only has been largely used.

The development of the cyanide process proceeded apace as soon as it had been introduced. The mechanical improvements devised were numerous, the most important being the enlargement of the size of the leaching vats, until the largest now hold 600 tons of ore. The chief chemical improvements have been the introduction of the use of

[1] *Jour. Chem. Soc.*, vol. lxiii., p. 724 (1893).

caustic soda or lime to neutralise the acids and acid salts in "weathered" pyritic ores, and the reduction in the strength of solutions, the favourite "strong" solution now containing from 0·25 to 0·30 per cent. of available KCy, while solutions as weak as 0·01 per cent. are found in many cases to be equally efficacious if a somewhat longer time is allowed. In accordance with the experience in the extraction mills MacLaurin[1] has found that gold and silver in the form of plates are most rapidly dissolved by solutions containing from 0·1 to 0·4 per cent. of cyanide, the maximum rate being observed with solutions containing about 0·25 per cent. of cyanide of potassium. A fairly rapid rate of dissolution however is still observable when only 0·005 per cent. of KCy is present.

Cyanide of potassium acts rapidly when free oxygen is present in large quantities, as for example when gold floats on the solution, with its upper side dry. Under such conditions, cyanide is at least as rapid in action as chlorine, but in proportion as the supply of free oxygen falls off, the rate of dissolution of gold in cyanide becomes slower, and when air is excluded as rigidly as possible, hardly any action can be observed. In practice these facts are of importance. Finely divided gold in ordinary ores is dissolved in two or three days. When concentrates containing a large proportion of sulphides are being treated, however, free oxygen is absorbed by the pyrites as well as by the solution, and the treatment lasts as much as two or three weeks with a corresponding increase in the destruction of the cyanide by the minerals in the ore. An artificial supply of oxygen or of oxidising agents shortens the time required, but increases the waste of cyanide. Moreover, when pyritic ores or concentrates contain much marcasite and, in many cases, when copper sulphides or some other minerals are present, the process is useless, enormous quantities of cyanide being converted into other compounds before any gold is dissolved.

The process is therefore of limited application to complex ores and concentrates generally, although of wide applicability to ores comparatively free from sulphides,

[1] *Jour. Chem. Soc., loc. cit.* and vol. lxvii., p. 199, 1895.

especially after the coarser particles of gold have been removed by amalgamation. Certain deposits of complex ores, above the average in richness, are at present left untouched for want of a process by which they can be treated.

The cost of treatment of gold ores by the processes described above of course varies enormously with the locality and the special conditions of the case. Under favourable conditions the following may indicate approximately the minimum amount of gold which must be present in an ore, in order that it may be treated at a profit by the most suitable process. Auriferous sand, washed in the miner's pan, must contain about one part of gold in 100,000 or say six pennyweights per ton of ore. When the sluice is used, the sand being excavated and carried to the sluice by hand labour, as in Siberia, there must be at least one part of gold in 2,500,000 parts of sand or six grains to the ton. When the hydraulic method is possible, only one part in 18,000,000 may be enough, or three-fourths of a grain of gold per ton, a proportion about equal to that which Liversedge[1] found to exist in the sea water off the east coast of Australia. If the gold is contained in quartz, the cost of mining and of crushing the stuff makes the whole process of treatment far more expensive. Excluding the cost of mining, however, the cost of crushing and amalgamation requires the presence of at least one part of gold in 400,000, or say one and a half dwts. per ton. The cost of chlorination is equal to not less than one part of gold in 200,000 of ore or three dwts. per ton, although if only applied to concentrates, the cost per ton of the original ore may be trifling. Lastly, the cyanide process might be applied to the crushed tailings from the amalgamation process if they contain more than one part of gold in 600,000 or one dwt. per ton. If the ore has to be crushed, it must of course be richer. As stated above, these estimates apply only when the conditions are the most favourable. In general, such poor ores could not be worked at a profit.

[1] *Proc. Roy. Soc. of N.S.W.*, 1895.

At the present time the annual production of gold from all these sources is about £42,000,000 per annum, or double the output of seven years ago. It is greater than at any previous period in history, the nearest approach to it having been made in 1853, when the river gravels of California and Australia were at their best. The output was then estimated at £38,000,000.

Of the present output, washing processes, formerly instrumental in producing by far the larger proportion of the gold won, are now probably answerable for no more than 30 per cent. The amalgamation of crushed vein stuff, now the most important process, produces about 55 per cent. of the whole gold production. The two wet processes, chlorination and cyanide, account for some 11 per cent. of the output, of which about two-thirds are contributed by the cyanide process, and, lastly, smelting perhaps produces 3 or 4 per cent. of the total. In the future, as placer gravels become exhausted more and more, the proportion of the output derived from them will continue to fall off, and the greatest increases will certainly take place in the produce from stamp battery amalgamation and the cyanide process. The output from chlorination and from smelting has been for some years stationary or declining, and their proportion of the world's production will doubtless suffer a continued though gradual reduction. It is obvious that by new discoveries the whole direction and progress of the art might be modified. As the old deposits become exhausted the miner is compelled to go deeper into the earth and the metallurgist to treat poorer and more refractory ores. "Such is the vast labour expended on the extraction of gold. And from this description I think it is clear that gold is hard to get as it is difficult to keep; and though all men long to get it, yet when they have it they find as much pain as pleasure in the use of it."[1]

T. K. Rose.

[1] Diodorus Siculus, book iii., chap. 12. Quoted by B. H. Brough, *loc. cit. antea.*

# THE MOST RECENT VALUES OF THE MAGNETIC ELEMENTS AT THE PRINCIPAL MAGNETIC OBSERVATORIES OF THE WORLD.

*From Data kindly supplied by Kew Observatory Committee.*

THE data in the table are deduced from hourly readings of the magnetic curves in the case of Pawlowsk, Katharinenburg, Copenhagen (Declination and Horizontal Force), Hamburg (Declination), Wilhelmshaven (Declination), Potsdam, Irkutsk, Utrecht (Declination), Kew, Greenwich, Uccle (Declination), Falmouth (Declination and Horizontal Force), Parc Saint Maur, Vienna, Pola, Nice (Declination), Perpignan, Tiflis, Washington, Zi-ka-wei, Manila, Batavia, and Mauritius.

In the case of Kasan (Declination and Horizontal Force), Prague (Declination and Horizontal Force), O'Gyalla (Declination and Horizontal Force), Madrid (Declination), Coimbra (Declination), and Lisbon (Declination), the diurnal inequality has been at least partly allowed for by the employment of readings at two or more hours of the day.

The first results of Inclination and Vertical Force at Greenwich are deduced from readings with 3-inch dip needles only, the second from readings with 3, 6, and 9 inch needles. Allowance has been made for a slight disturbance through recent building, in accordance with data kindly supplied by the Astronomer Royal.

The Declination at Kasan in last year's table was erroneously given as West.

| Place. | Latitude. | Longitude. | Year. | Declination. |
|---|---|---|---|---|
| Pawlowsk - - | 59° 41′ N. | 30° 29′ E. | 1894 | 0° 10′·5 E. |
| Katharinenburg - | 56° 49′ N. | 60° 38′ E. | 1894 | 9° 39′·4 E. |
| Kasan - - - | 55° 47′ N. | 49° 8′ E. | 1892 | 7° 30′·8 E. |
| Copenhagen - - | 55° 41′ N. | 12° 34′ E. | 1893 | 10° 47′·7 W. |
| Stonyhurst - - | 53° 51′ N. | 2° 28′ W. | 1895 | 18° 37′·8 W. |
| Hamburg - - | 53° 34′ N. | 10° 3′ E. | 1895 | 11° 42′·7 W. |
| Wilhelmshaven - | 53° 32′ N. | 8° 9′ E. | 1895 | 12° 52′·5 W. |
| Potsdam - - - | 52° 23′ N. | 13° 4′ E. | 1891 | 10° 42′·2 W. |
| Irkutsk - - - | 52° 16′ N. | 104° 16′ E. | 1894 | 2° 8′·0 E. |
| Utrecht - - - | 52° 5′ N. | 5° 11′ E. | 1893 | 14° 28′·5 W. |
| Kew - - - | 51° 28′ N. | 0° 19′ W. | 1895 | 17° 16′·8 W. |
| Greenwich - - | 51° 28′ N. | 0° 0′ | 1895 | 16° 57′·4 W. |
| Uccle (Brussels) - | 50° 48′ N. | 4° 20′ E. | 1893 | 14° 48′·7 W. |
| Falmouth - - | 50° 9′ N. | 5° 5′ W. | 1895 | 18° 54′·5 W. |
| Prague - - - | 50° 5′ N. | 14° 25′ E. | 1895 | 9° 31′·5 W. |
| Parc St. Maur (Paris) | 48° 49′ N. | 2° 29′ E. | 1893 | 15° 21′·1 W. |
| Vienna - - - | 48° 15′ N. | 16° 21′ E. | 1894 | 8° 43′·6 W. |
| O'Gyalla (near Buda Pesth) - - - | — | — | 1894 | 7° 58′·2 W. |
| Pola (on Adriatic) - | 44° 52′ N. | 13° 50′ E. | 1895 | 9° 47′·0 W. |
| Nice - - - | 43° 43′ N. | 7° 16′ E. | 1893 | 12° 32′·7 W. |
| Toronto - - - | 43° 40′ N. | 79° 30′ W. | 1894 | 4° 43′·9 W. |
| Perpignan - - | 42° 42′ N. | 2° 53′ E. | 1893 | 14° 10′·5 W. |
| Rome - - - | 41° 54′ N. | 12° 27′ E. | 1891 | 10° 45′·1 W. |
| Tiflis - - - | 41° 43′ N. | 44° 48′ E. | 1893 | 1° 38′·0 E. |
| Madrid - - - | 40° 25′ N. | 3° 40′ W. | 1893 | 16° 14′·2 W. |
| Coimbra - - - | 40° 12′ N. | 8° 25′ W. | 1893 | 17° 51′·7 W. |
| Washington - - | 38° 53′ N. | 77° 0′ W. | 1892 | 4° 14′·2 W. |
| Lisbon - - - | 38° 43′ N. | 9° 9′ W. | 1893 | 17° 49′·4 W. |
| Zi-ka-wei - - - | 31° 12′ N. | 121° 26′ E. | 1894 | 2° 16′·5 W. |
| Hong-Kong - - | 22° 18′ N. | 114° 10′ E. | 1894 | 0° 29′·2 E. |
| Colaba - - - | 18° 54′ N. | 72° 49′ E. | 1894 | 0° 38′·6 E. |
| Manila - - - | 14° 35′ N. | 127° 11′ E. | 1894 | 0° 50′·4 E. |
| Batavia - - - | 6° 11′ S. | 106° 49′ E. | 1894 | 1° 27′·6 E. |
| Mauritius - - | 20° 6′ S. | 57° 33′ E. | 1893 | 10° 2′·1 W. |
| Melbourne - - | 37° 50′ S. | 144° 58′ E. | 1894 | 8° 13′·6 E. |

| Place. | Year. | Inclination. | Horizontal Force, C.G.S. Units. | Vertical Force, C.G.S. Units. |
|---|---|---|---|---|
| Pawlowsk - - | 1894 | 70° 43′·6 N. | ·16456 | ·47061 |
| Katharinenburg - | 1894 | 70° 40′·0 N. | ·17799 | ·50729 |
| Kasan - - - | 1892 | 68° 36′·2 N. | ·18551 | ·47345 |
| Copenhagen - - | 1893 | 68° 51′·0 N. | ·17358 | ·44868 |
| Stonyhurst - - | 1895 | 68° 59′·2 N. | ·17148 | ·44637 |
| Hamburg - - | 1895 | 67° 44′·3 N. | ·18009 | ·43994 |
| Wilhelmshaven - | 1895 | 67° 54′·5 N. | ·17983 | ·44305 |
| Potsdam - - | 1891 | 66° 44′·1 N. | ·18635 | ·43342 |
| Irkutsk - - - | 1894 | 70° 10′·5 N. | ·20116 | ·55796 |
| Utrecht - - | 1893 | 67° 12′·2 N. | ·18397 | ·43772 |
| Kew - - - | 1895 | 67° 23′·8 N. | ·18278 | ·43901 |
| Greenwich - - | 1895 | { 67° 15′·9 N. / 67° 14′·9 N. } | ·18323 | { ·43727 / ·43692 } |
| Uccle - - - | 1893 | 66° 28′·4 N. | ·1877 | ·4311 |
| Falmouth - - | 1895 | 67° 0′·4 N. | ·18547 | ·43708 |
| Prague - - - | 1895 | — | ·19834 | — |
| Parc St. Maur - | 1893 | 65° 7′·1 N. | ·19621 | ·42304 |
| Vienna - - - | 1894 | 63° 12′·1 N. | ·20740 | ·41061 |
| O'Gyalla - - | 1894 | — | ·21054 | — |
| Pola - - - | 1895 | 60° 34′·0 N. | ·22026 | ·39038 |
| Nice - - - | 1893 | 60° 26′·4 N. | ·22198 | ·39139 |
| Toronto - - | 1894 | 74° 35′·0 N. | ·16624 | ·60286 |
| Perpignan - - | 1893 | 60° 11′·9 N. | ·22304 | ·38944 |
| Rome - - - | 1891 | 58° 4′·6 N. | ·2324 | ·3730 |
| Tiflis - - - | 1893 | 55° 45′·7 N. | ·25692 | ·37751 |
| Madrid - - - | 1893 | — | — | — |
| Coimbra - - | 1893 | 59° 50′·5 N. | ·22518 | ·38752 |
| Washington - - | 1892 | 71° 3′·9 N. | ·19848 | ·57858 |
| Lisbon - - - | 1893 | 58° 24′·6 N. | ·23270 | ·37840 |
| Zi-ka-wei - - | 1894 | 46° 0′·7 N. | ·32613 | ·33785 |
| Hong-Kong - - | 1894 | 31° 53′·1 N. | ·36450 | ·22675 |
| Colaba - - - | 1894 | 20° 40′·7 N. | ·37426 | ·14126 |
| Manila - - - | 1894 | 16° 54′·3 N. | ·37740 | ·11470 |
| Batavia - - - | 1894 | 29° 13′·7 S. | ·36749 | ·20563 |
| Mauritius - - | 1893 | 54° 44′·3 S. | ·23989 | ·33929 |
| Melbourne - - | 1894 | 67° 16′·9 S. | ·23426 | ·55956 |

# APPENDIX I.

## NOTICES OF BOOKS.

*Studies in the Evolutionary Psychology of Feeling.* By Hiram M. Stanley. London: Swan Sonnenschein & Co. New York: Macmillan & Co., 1895. Pp. viii., 392.

The chief idea pervading the book is the importance of mind in evolution. The reasoning starts from the assumption that the primitive form of consciousness was pain, and pain is held to have been the essential factor in the evolution of mind by means of its stimulating action on volition. Not only the higher mental developments, but even the senses are supposed to owe their existence to conscious struggle in the effort to avoid pain. The course of evolution is supposed to have been pain—pleasure—vague cognition of object—sensation. In considering the emotions, of which fear is held to be the most primitive, the author does not believe in their gradual evolution, but supposes that such an emotion as anger appeared discontinuously in some favoured individual that derived advantage from the power of "getting mad" and violently attacking its fellows. The book is not scientific in method, but contains some useful discussions of psychological questions.

*Milk; Its Nature and Composition.* A Handbook on the Chemistry and Bacteriology of Milk, Butter and Cheese. By C. M. Aikman, M.A., D.Sc. London: Adam & Charles Black, 1895. Pp. xiv., 173. Price 3s. 6d.

In the space of less than 200 pages Dr. Aikman has contrived to give a good general account of the more important facts concerning milk—a valuable food which it behoves the general public no less than the milk purveyor and the dairy farmer to thoroughly understand.

Many persons having charge of children and invalids who subsist largely on milk have but a scanty knowledge of its composition and peculiarities: to these as well as to those in charge of dairy farms Dr. Aikman's book will prove of value, as in addition to a good description of the perfect article, the "faults" of milk, the bacteria found in it, and the food stuffs prepared from it are fully treated.

The value of the work is increased by the presence of well-selected figures in the text, and a short list of works on dairying in an appendix.

*Handbuch der paläarktischen Gross-Schmetterlinge für Forscher und Sammler. Zweite gänzlich umgearbeitete und durch Studien zur Descendenztheorie erweiterte Auflage des Handbuches für Sammler der europäischen Gross-Schmetterlinge.* Von Dr. M. Standfuess, Dozent beider Hochschulen und Kustos des Entomologischen Museums am eidgen. Polytechnikum zu Zürich, mit 8 lithographischen Tafeln und 8 Textfiguren. Jena: Gustav Fischer, 1896, 8vo, pp. xii., 392.

This book is a curious medley, combining several branches of entomology which we are seldom accustomed to see discussed in the same work, at least in England. The author complains in his preface that too many entomologists are mere collectors, or at least concern themselves only with questions of species; and therefore he has made his work not only a comprehensive handbook of the formation and management of a collection, but has added large sections relating to variation, dimorphism, hybridism, and other subjects connected with the origin of species, illustrated with eight coloured plates. Most of the varieties, hybrids, etc., represented belong to well-known British species. The more scientific portion of the book is in large measure an addition to the contents of the earlier and smaller edition.

The author is by no means satisfied with the present position of entomology among the sciences, which he attributes to the already mentioned neglect of scientific entomology among collectors. Apparently he hopes to contribute to an improvement in this direction, and he thus expresses his views in the preface:—

"This is the principal reason why the technical entomological literature of the time is almost totally neglected by scientific zoology, and ignored. In future it must not be thus.

Entomology must not be treated as a stepchild and Cinderella, to be neglected by her proud sister, but must work shoulder to shoulder with her as a faithful and equal comrade, carrying stones for the building which inquirers are endeavouring by honest work, upon a true knowledge of nature, to raise up as a harmonious whole."

*A Handbook of British Lepidoptera.* By Edward Meyrick, B.A., F.Z.S., F.E.S., Assistant Master at Marlborough College. London and New York: Macmillan & Co., 1895. 8vo, pp. vi., 843.

In this compact and closely-printed volume Mr. Meyrick has given us a convenient Students' Manual of British Butterflies and Moths, which was greatly wanted by all collectors who had outgrown the numerous popular books, which, as a rule, include only the butterflies and larger moths, the far more numerous "Micro-Lepidoptera" being omitted. Mr. Meyrick's work, however, includes all these in a single volume, not, of course, giving complete information on every point, but short descriptions and tables of genera and species, and notices of larvæ, times of appearance, and localities. We have many similar handbooks of botany and ornithology, and it is rather surprising that this is almost the first of its kind as regards *Lepidoptera.* There are no illustrations except woodcuts of neuration, a character to which Mr, Meyrick attaches a perhaps somewhat exaggerated importance. Enough has been and will be written elsewhere on the new classification of the *Lepidoptera* proposed by Mr. Meyrick, and we need only here allude to the fact of its being totally dissimilar to that adopted by any other entomologist.

Catalogue of the Mesozoic Plants in the Department of Geology, British Museum (Natural History). *The Wealden Flora.* Part II., "Gymnospermæ". With twenty Plates and nine Figures in the Text. By A. C. Seward, M.A., F.G.S. London: 1895.

Mr. Seward, of Cambridge, has for some time past been engaged on an examination of the fossil plants from the Wealden beds, contained in the collections of the British Museum. The material on which the investigation is based was for the most part collected by Mr. Rufford, whose valuable specimens have been acquired by the Museum. Mr. Seward's first volume, containing the "Cryptogams," appeared in 1894; the part now published completes the work, for, unfortunately, the English Wealden has not yet yielded any Angiospermous remains, although both Monocotyledons and Dicotyledons have been found in beds of similar horizon abroad.

The present volume is concerned with the two orders Cycadaceæ and Coniferæ. The author, however, points out that we are still to a large extent in the dark as to the exact nature and structure of extinct Cycadean plants. Beautiful as many of the specimens are, and striking as is the similarity of their organs to those of existing Cycads, we can seldom be certain that we have to do with Cycadaceæ, in the sense of recent Botany. There is nearly always the possibility that the remains may rather belong to the extinct family Bennettiteæ, allied to the Cycads, but deviating widely from them in the structure of the reproductive organs. Mr. Seward appears to recognise one species only (among those recorded in this book) as representing a truly Cycadean flower. This is his *Androstrobus Nathorstii*, which seems to be beyond doubt a male cone of the true Cycadean type. The other fructifications described are regarded as "*incertæ sedis*," or transferred to the Coniferæ, or else they belong unmistakably to the *Bennettites* type. This latter fructification, so thoroughly known from the researches of Carruthers, Solms-Laubach, and Lignier, is proved by the author to be well represented in the Wealden strata. He founds a new species—*Bennettites Carruthersi*—for some very fine specimens, which exhibit in great perfection all the more external characters of this extraordinary fructification. Mr. Seward identifies *Bennettites* with the famous *Williamsonia*, and inclines to the view that all the specimens which have been satisfactorily determined represent female inflorescences. Another species of *Bennettites* illustrates very finely the way in which the inflorescences were borne on the stem.

Certain Wealden stems had been referred by previous writers to *Dracæna.* The author rejects this determination, and shows that they bear a much greater likeness to certain Cycadean stems, especially those of some species of *Zamia.*

A most anomalous fossil, of uncertain affinities, is placed by Mr. Seward in a new genus—*Withamia.* It consists of a woody axis, bearing very large recurved spines, in the axils of which leaf-like organs, somewhat suggestive of a *Gingko*, are seated. Nothing like this is known among living plants, though *Phyllocladus* presents certain analogies.

Many beautiful coniferous specimens are recorded, from which it would appear (though the author here, as indeed everywhere, expresses himself with the most admirable caution) that most tribes of Coniferæ were already represented in the Wealden epoch. A new species of *Pinites* (*P. Solmsi*) shows both foliage and female cones very clearly, and decidedly suggests the recent genus, while in *Sphenolepidium Kurrianum* we have equally perfect specimens of another type, indicating an affinity to *Athrotaxis.*

The author considers that the evidence of Palæobotany certainly favours the inclusion of the Wealden rocks in the Jurassic series.

Such work as Mr. Seward's is of the greatest possible value. His book, with the help of the abundant and excellent illustrations (chiefly the work of Miss Woodward), gives us a vivid idea of the flora of a most interesting epoch. The author's remarkably sound judgment, and caution in estimating affinities, inspire great confidence in his results. It is only by such sober-minded work as this that real and permanent advance in fossil Botany can be assured.

*Die Artbildung und Verwandschaft bei den Schmetterlingen.* II. Theil. Eine systematische Darstellung der Abänderungen, Abarten und Arten der Schwalbenschwanz-ähnlichen Formen der Gattung Papilio. Von Dr. G. H. Theodor Eimer, Professor der Zoologie und vergleichenden Anatomie zu Tübingen. Unter Mitwirkung von Dr. K. Fichert, I. Assistent an der Zoologischen Anstalt daselbst. Mit 4 Tafeln in Farbendruck und 7 Abbildungen im Texte. Jena, 1895.

Times have changed since it was possible to regard a fondness for butterfly collecting as evidence which could be seriously brought forward in a court of justice in an attempt to set aside the will of a deceased lady on the ground of insanity. Gone, too, are the days when it was necessary for pious and learned gentlemen to explain to our fathers in long and elaborate essays that Entomology was not in itself either a frivolous or a cruel amusement. Yet the really cruel amusements of the beginning of the century have disappeared and are forgotten, while Entomology still holds its own, and to judge from the large number of books addressed chiefly to amateurs, is an increasingly popular amusement, while wise men are turning their attention to the despised butterflies in quest of information respecting some of the profoundest problems which are open to our scientific men along the present tracks of ordinary research.

It was Bates' famous paper on Mimicry which first began to raise the philosophical study of butterflies to the importance which it has now attained, and questions of mimicry, variation, adaptation, climate, and even of the past history of the world, and of the formation of species are eagerly discussed in the new light which the study of butterflies has thrown upon them.

Not only long and extended but much detailed examination is necessary before satisfactory results can be attained; and therefore works of a limited character like Dr. Eimer's[1] will always be important, quite apart from any theories which may be based upon them. Dr. Eimer contends that Natural Selection will not account for the origin of species, though it doubtless contributes largely to the preservation of species that are already formed. He also adduces instances in which the representative forms of a species or group of species appear to vary in a similar direction at the opposite extremities of its range; but perhaps his most important point is that variation always appears to occur along definite lines. We are glad to notice this, because, although it has frequently been noticed, it has generally been passed by without special comment. It would be impossible to discuss Dr. Eimer's theories at length; but we are sure that all who are interested in the philosophical study of butterflies will read his book with much interest.

[1] The present volume of his work is restricted to *Papilio Turnus*, *Machaon*, and *Asterias*, and their allies.

# APPENDIX II.

## CHEMICAL LITERATURE FOR JANUARY, 1896.

### Vol. i. No. 2. *American Journal of Science.* (February, 1896.)

*Walker, T. L.*, Notes on Sperrylite (pp. 110-113). *Penfield, S. L.*, and *Forbes, E. H.*, Fayalite from Rockport, Mass., and on the Optical Properties of the Chrysolite-Fayalite Group, and of Monticellite (pp. 129-136).

### Vol. xviii. No. 1. *Journal of the American Chemical Society.* (January, 1896.)

*Heidenhain, H.*, On the Determination of Carbon Dioxide by Absorption (pp. 1-8). *Cross, C. F.*, *Bevan, E. J.*, and *Beadle, C.*, The Natural Oxycelluloses (pp. 8-21). *Gill, A. H.*, and *Richardson, H. A.*, Notes on the Determination of Nitrites in Potable Water (pp. 21-23). *Gladding, T. S.*, A Gravimetric Method of Estimating Phosphoric Acid as Ammonium Phosphomolybdate (pp. 23-28). *Flintermann, R. F.*, and *Prescott, A. B.*, Dipyridine Trimethylene Dibromide and a Study of Certain Additive Reactions of Organic Bases (pp. 28-35). *Campbell, E. D.*, A Proposed Schedule of Allowable Difference and of Probable Limits of Accuracy in Quantitative Analyses of Metallurgical Materials (pp. 35-38). *Pennington, M. E.*, Derivatives of Columbium and Tantalum (pp. 38-67). *Gill, A. H.*, An Improved Gas Pipette for the Absorption of Illuminants (pp. 67-68). *Richards, E. H.*, and *Ellms, J. W.*, The Colouring Matter of Natural Waters; its Source, Composition and Quantitative Measurement (pp. 68-81). *Wiley, H. W.*, On the Estimation of Levulose in Honeys and other Substances (pp. 81-91). *Prescott, A. B.*, Notes on a few Pyridine Alkyl Iodides (pp. 91-96). *Rising, W. B.*, and *Lenher, V.*, An Electrolytic Method for the Determination of Mercury in Cinnabar (pp. 96-98).

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*Jackson, J. C. L.*, and *Oenslager, G.*, On the Constitution of Phenoquinone (pp. 1-23). *Warder, R. B.*, The Mathematical Theory of Oxidation Processes (pp. 23-43). *Mabery, C. F.*, On the Composition of the Ohio and Canadian Sulphur Petroleums (pp. 43-80).

### Vol. xxi. No. 239. *The Analyst.* (February, 1896.)

President's Annual Address (pp. 29-36).

### Vol. lxix. No. 399. *Journal of the Chemical Society.* (February, 1896.)

*Marsh, J. E.*, and *Gardner, J. A.*, Researches on the Terpenes. VI. Products of the Oxidation of Camphene, Camphoic Acid and its Derivatives (pp. 74-91). *Cohen, J. B.*, and *Archdeacon, W. H.*, The Action of Sodium Alcoholate on the Acid Amides (pp. 91-96). *Ewan, T.*, Note on the Electrolytic Conductivity of Formanilide and Thioformanilide (pp. 96-98). *Snape, H. L.*, On Certain Phenylthiocarbamates (pp. 98-102). *Shaw, G. E.*, Periodides of Theobromine (pp. 102-104). *Frankland, P. F.*, and *MacGregor, J.*, Ethereal Salts of Active and Inactive Monobenzoyl-, Dibenzoyl-, Diphenacetyl- and Dipropionyl-Glyceric Acids (pp. 104-123). *Frankland, P. F.*, and *Pickard, R. H.*, Rotation of Optically Active Compounds in Organic Solvents (pp. 123-142). *Nicol, W. W. J.*, The Molecular Volumes of Organic Substances in Solution (pp. 142-145).

### Vol. xli. No. 249. *Philosophical Magazine and Journal of Science.* (Feb., 1896.)

*Wood, R. W.*, On the Dissociation Degree of some Electrolytes at 0° (pp. 117-120). *Wood, R. W.*, The Duration of the Flash of Exploding Oxyhydrogen (pp. 120-123).

### Vol. lix. No. 353. *Proceedings of the Royal Society.* (15th January, 1896.)

*Lockyer, J. N.*, On the Gases obtained from thc Mineral Eliasite (pp. 1-4). *Lockyer, J. N.*, On the New Gases obtained from Uraninite. VI. (pp. 4-9). *Kuenen, J. P.*, and *Randall,*

*W. W.*, The Expansion of Argon and of Helium as compared with that of Air and Hydrogen (pp. 60-66). *Kellas, A.*, On the Percentage of Argon in Atmospheric and in Respired Air (pp. 66-68). *Kellas, A.*, and *Ramsay, W.*, Examination of Gases from certain Mineral Waters (pp. 68-69). *Lundström, C. J.*, Flame Spectra observed at Swedish Bessemer Works (76-98). *Hartley, W. N.*, Remarks on the Origin of some of the Lines and Bands observed in the Spectra from Swedish Bessemer Works (pp. 98-101).

Series vii. Tome vii. *Annales de Chimie et de Physique.* (February, 1896.)

*Marie, T.*, Recherches sur les acides cérotique et mélissique (pp. 145-251). *Louguinine, W.*, Étude sur les chaleurs latentes de vaporisation des liquides (pp. 251-283). *Berthelot*, Sur la détermination thermochimique de l'équivalent des acides et des bases (pp. 283-288).

Tomes xv.-xvi. No. 2. *Bulletin de la Société Chimique de Paris.* (20th January, 1896.)

*Lenoble, E.*, Sur le nouveau mode de représentation des courbes de solubilité des sels, proposé par M. Etard (pp. 54-58). *Rousset, L.*, Sur quelques acétones dérivées du naphtalène (pp. 58-72). *Cazeneuve, P.*, Recherches sur la décomposition des acides-phénols dérivés du benzène et du naphtalène (pp. 72-82). *Lauth, C.*, Sur quelques dérivés dithiazoliques (pp. 82-87). *Grimbert, L.*, Fermentations provoquées par le pneumobacille de Friedlander (pp. 87-96). *Béchamp. A.*, Sur les altérations spontanées du lait et sur celles que la cuisson lui fait subir (pp. 96-118). *Lasne, H.*, Sur le dosage de l'alumine dans les phosphates (pp. 118-128).

Tomes xv.-xvi. No. 3. (5th February, 1896.)

*Brunel, H.*, Sur l'acide thioglyoxylique (éthanethioloïque) (pp. 134-135). *Bredt, J.*, et *de Rosenberg*, Synthèse partielle du camphre (pp. 135-142). *Prud'homme, M.*, Nouvelle synthèse de la parafuchsine et de ses dérivés mono-, di-, tri- et tetraalcoylés (pp. 142-146). *Lasne, H.*, Sur le dosage de l'alumine dans les phosphates. II. (pp. 146-158). *Colson, A.*, Essai sur le dosage polarimétrique de l'acide tartrique (pp. 158-162). *Winter, J.*, Note additionelle à un mémoire paru dans le *Bulletin de la Société Chemique* (3e serie, t. xiii., pp. 1101-1895), sur la température de congélation des liquides de l'organisme (pp. 162-163).

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*Thomas, V.*, Action du peroxyde d'azote sur les sels halogénés d'étain (pp. 32-34). *Oechsner de Coninck*, Sur un mode de décomposition de quelques composés à fonction amide ou basique (pp. 34-35).

Tome cxxii. No. 2. (13th January, 1896.)

*Amagat, E. H.*, Sur les variations du rapport des deux chaleurs spécifiques des gaz (pp. 66-70). *Violle, J.*, Un étalon photométrique à l'acétylène (pp. 79-80). *Le Chatelier, H.*, Sur la chaleur de formation de quelques composés du manganèse (pp. 80-82). *Tassilly*, Sur les iodures cristallisés de strontium et de calcium (pp. 82-84). *Barbier, P.*, et *Bouveault, L.*, Sur les aldéhydes dérivées des alcools $C_{10}H_{18}O$ isomériques (pp. 84-86). *Tanret*, Sur la multirotation des sucres redacteurs et l'isodulcite (pp. 86-87).

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*François, M.*, Action de la chaleur sur l'iodure mercureux (pp. 190-193). *Bayrac* et *Camichel, C.*, Sur l'absorption de la lumière par les dissolution d'indophénols (pp. 193-195). *Perrier, G.*, Combinaisons du chlorure d'aluminium anhydre avec les phénols et leurs dérivés (pp. 195-198). *Bouchardat, G.*, et *Tardy*, Sur l'essence d'anis de Russie (pp. 198-201). *Brochet, A.*, Sur la production de l'aldéhyde formique gazeuse pure (pp. 201-203).

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*Bourquelot* et *Bertrand, G.*, Les ferments oxydants dans les champignons (pp. 97-102). *Riche, A.*, Conservation des peaux par les préparations arsenicales (pp. 102-106). *Meillère, G.*, Essai des acides nitrique et chlorhydrique (pp. 106-107). *Marie, T.*, Sur les propriétés des acides cérotique et mélissique existant à l'état libre dans la cire d'abeilles (pp. 107-111). *Barthe, L.*, Analyse de concrétions intestinales (pp. 111-113). *Rebière, G.*, Sur un nouveau mode de dosage des benzoates alcalins (pp. 113-116). *Chicote, C.*, Une nouvelle falsification du safran (pp. 116-117). *Fouquet, L.*, Sur un calcul biliare contentant de l'acide stéarique (pp. 117-119).

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*Erlenmeyer, E.*, Ueber Phenyldihalogenpropionsäuren, insbesondere ueber Phenylchlorjodpropionsäure und einige Derivate derselben (pp. 259-285.) *Hantzsch, A.*, und *Wild, W.*, Ueber Oxime aus α-halogenisirten Aldehyden, Ketonen und Säuren sowie ueber Oximessigsäuren (pp. 285-310). *Smith, A.*, Ueber die Einwirkung von Hydrazin und von Phenylhydrazin auf 1,4 Diketone (pp. 310-337). *Wallach, O.*, Zur Kenntniss der Terpene und der ätherischen Oele (pp. 337-362). *Beckmann, E.*, Untersuchungen in der Kampherreihe (pp. 362-367). *Beckmann, E.*, und *Mehrländer, H.*, Zur Kenntniss der Menthone (pp. 367-391).

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*Jackson, C. L.*, und *Ittner, M. H.*, Ueber Parabromdimetanitrotoluol und enige seiner Derivate (pp. 3063-3066). *Jackson, C. L.*, und *Phinney, J. J.*, Notiz ueber den Trinitrophenylmalonsäureester (pp. 3066-3068). *Ladenburg, A.*, Ueber Lysidin (pp. 3068-3070). *Schuftan, A.*, Ueber αμ-Dimethyloxazol (pp. 3070-3072). *Schneider, P.*, Ueber einige Alkylderivate des Aethylendiamins (pp. 3072-3078). *Lobry de Bruyn, C. A.*, und *van Ehenstein, A. W.*, Einwirkung von Alkalien auf Kohlenhydrate. Wechselseitige Umsetzung von Glucose, Fructose und Mannose in einander (pp. 3078-3082). *Lobry de Bruyn, C. A.*, Ueber die Ammoniakderivate der Kohlenhydrate (pp. 3082-3085). *Lobry de Bruyn, C. A.*, Ueber das freie Hydrazin (pp. 3085-3086). *Lobry de Bruyn, C. A.*, Darstellung und einige Eigenshaften des Hydrazinhydrats (pp. 3086-3087). *Hjelt, E.*, Ueber den Sogen. Ledumcampher (pp. 3087-3089). *Möhlau, R.*, und *Krübel, F.*, Ueber 1·2-Dioxy-3-naphtoësäure (pp. 3089-3096). *Möhlau, R.*, Ueber 2·3-Amidonaphtoësäure (pp. 3096-3100). *Möhlau, R.*, Zur Constitutionsfrage der 2·3-Oxynaphtoësäure und ihrer Derivate (pp. 3100-3102). *Landolt, H.*, Ueber eine veränderte Form des Polarisationsapparates für Chemische Zwecke (pp. 3102-3104). *Ladenburg, A.*, Erwiderung (pp. 3104-3106). *Abel, J.*, Ueber α-Naphtylpiperidin (3106-3111). *Bookman, S.*, Ueber β- und γ-Aethoxybutylamin (pp. 3111-3121). *Zincke, Th.*, Ueber eine neue Reihe von Chinonartigen Derivaten (pp. 3121-3127). *Wegscheider, R.*, Ueber die Esterbildung aus Säure und Alkohol (pp.

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*Schunck, E.*, und *Marchlewski, L.*, Zur Kenntniss des Isatins (pp. 194-203). *Von Lippmann, E. O.*, Bemerkung zur Frage ueber die Ursache der Birotation (pp. 203-204). *Scheitel, A.*, Darstellung der Salze der Platincyan-Wasserstoffsäure $Pt(CN)_4H_2$. (pp. 204-205). *Fischer, E.*, Neue Bildungsweise der Oxazole (pp. 205-214). *Zelinsky, N.*, und *Reformatzky, A.*, III. Untersuchungen in der Hexamethylenreihe (Synthese des Nononaphtens) (pp. 214-216). *Heffter, A.*, Ueber Cacteen-alkaloïde (pp. 216-228). *Kerp, W.*, Ueber Diphenylenketon und Pseudo-Diphenylenketon (pp. 228-233). *Bablich, A.*, und *v. Kostanecki, St.*, Ueber Oxybenzalacetophenone (pp. 233-237). *V. Kostanecki, St.*, und *Tambor J.*, Ueber $\alpha$-Cumaryl-Phenylketon (pp. 237-240). *Cornelson, A.*, und *v. Kostanecki, St.*, Zur Einwirkung der Aldehyde auf Ketone (pp. 240-244). *V. Kostanecki, St.*, und *Oppelt, E.*, Ueber einige Derivate des 2-Oxy-benzalacetophenons (pp. 244-249). *Knorr, L.*, Ueber das Pyrazolon (pp. 249-260). *Bleier, O.*, Ueber gasanalytische Apparate (pp. 260-265). *Schöpf, M.*, Zur Constitutionsfrage der 2·3-Oxynaphtoësaure (pp. 265-270). *Schall, C.*, Ueber $\gamma$-Carbodiphenylimid (pp. 270-272). *Bamberger, E.*, und *Kraus, E.*, Ueber Thiodiazoverbindungen (pp. 272-287). *Schraube, C.*, und *Fritsch, M.*, Wanderungen der Diazogruppen (pp. 287-295). *Erlenmeyer, E.*, Zur Kenntniss der Diphenyloxäthylaminbasen (pp. 295-298). *Schiff, H.*, Biuret-reactionen (pp. 298-303). *Gabriel, S.*, und *Stelzner, R.*, Ueber (B, 3-) Methylindazol (pp. 303-310). *Klages, A.*. Ueber einige Derivate des m-Xylols (pp. 310-314). *Jünger, E.*, und *Klages, A.*, Ueber den Abbau des Menthols (314-318).

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# APPENDIX I.

## NOTICES OF BOOKS.

*Die Spiele der Thiere.* By Karl Groos. Jena: G. Fischer, 1896. Pp. xvi., 359.

The chief point which is worked out in this book is that animal play depends on a deeply planted instinct of the greatest importance in the struggle for existence. Spencer's view that play is the expression of overflow of energy is held to be only a small part of the truth. Play is regarded as the instinctive performance, without real cause, of actions resembling those useful in the actual struggle for life. Superfluous energy is the most favourable condition for play, but the author points out that the impulse to play is so strong that animals will react to a stimulus to play even when in an exhausted condition.

The second chapter contains a good account of the views which have been held on the subject of instinct, the author's own position being very close to that of Weismann. Play being regarded as an instinct, the play of young animals is naturally most fully considered. The importance of play is rated so highly that the author supposes that "youth owes its existence to the necessity for play". In the next two chapters, in which the games of animals are considered in detail, the author's views are illustrated by copious examples drawn from very wide sources, but chiefly from the accounts of those who have observed animals in a state of nature. One of the most quoted authors in this respect is Hudson. The simplest kind of play is called "experimenting," and this term is employed for the actions of young animals, by means of which they obtain command over their own movements and over external objects. Under the heading of play of movement are included most of the examples quoted by Spencer in support of his theory. Hunting and fighting games are fully considered, and they give strong support to the author's view. In considering such play as that of a cat with a caught mouse, strong objection is made to Romanes' view that animals delight in torture for torture's sake; such play is rather regarded as instinctive activity adapted to improve skill. Instances are given in which building and nursing appear to have occurred in play. Imitation games are considered, and imitation is regarded as an instinct closely associated with the play instinct.

The performances of courtships are considered separately, differing from other kinds of play in that they have a real occasion. The subject of sexual selection is fully discussed. The author is sceptical on the question of conscious choice by the female, and he thinks that there is distinct evidence that pairing takes place before the courtship performances begin. He regards it as necessary for the preservation of the species that there should be some restraint of the sexual act, and he supposes that this restraint is provided by instinctive coyness of the female, and that the performances are carried out in order to overcome this coyness by producing sexual excitement; coquetry is the result of the struggle of two opposed instincts. A full account is given of the various forms which these performances may take, and instances are given in which they have been observed in young animals.

In the final chapter the psychology of play is considered. The games of young animals are held to be purely instinctive, and their only mental accompaniment to be the pleasure attending the satisfaction of an instinct. In the adult animal it is believed that play is often accompanied by consciousness of the unreal nature of the activity, the chief element in this consciousness being the pleasure of power. Instances of dissimulation in animals are quoted in support of the existence of consciousness of sham-occupation. In conclusion, the relation of animal play to the various forms of art is dealt with, and this part of the subject will no doubt be more fully treated in another work on human play which is promised by the author.

*Text-book of the Embryology of Invertebrates.* By E. Korschelt and K. Heider. Translation into English by E. L. Mark and W. Mc. M. Woodworth, with additions by the Authors and Translators. Part I. London: Swan Sonnenschein & Co., Limited. New York: Macmillan & Co., 1895.

This volume of 466 pp., with 225 illustrations, has been eagerly awaited. The original is too familiar to those who read German to need comment here; suffice it to say that it deals in a lucid manner with the development of the Invertebrata, including the Enteropneusta and Rotatoria, with the exception of the Mollusca, Brachiostoma, and Arthropoda. Prof. Mark's

previous translations are so well known, and the work which for years has appeared under his direction in the *Bull. Mus. Comp. Zool.* at Harvard College has become so famous for the thoroughness of its bibliographical department, that the success of the present venture seemed assured in his hands.

The original is certainly the best general treatise on Embryology which has appeared since Balfour's, if indeed it is not in some respects preferable to that. With respect to the share taken by the translators in bringing it up to date, comparison arises with the senior translator's rendering of Hertwig's *Text-book of Embryology*, in which a similar resolve was made but very inefficiently carried out. Not so here; and in the selection of new matter, a wise discretion has been exercised in the presentation of novelties to the student mind, to wit, in the treatment of those observations tending towards the overthrow of the germ-layer theory. We note that due regard has been paid to the various modes of asexual reproduction and regeneration, a feature not always met with in works on Embryology; but, conversely, we regret that Chun's *Dissogonie* has not received the attention it deserves in the Chapter on the Ctenophora. And it is a distinctly healthy sign to read in a text-book of pure Embryology, apropos of Semon's "*Pentactula*" stage in Echinoderm development and a discussion of its possible bearings on Phylogany, that it seems "more justifiable to search for the ancestral forms of the Echinodermata among the existing material which is offered us by Palæontology".

Of the success of the translation and of the future of the book there can be no doubt. It is clear in style and in get up. All has been done with a due sense of proportion, and the few errors we have detected are such as will be self-evident to the intelligent reader. We can confidently recommend the work as the most generally serviceable on the subject in the English tongue.

The translators remark in their preface that they have been "compelled by the pressure of other duties to relinquish to others the task" of translation of the two remaining parts of the work. In this they have become notorious, for, loyal to an earlier suggestion of the senior translator, they have rendered "anlage" as "fundament"! The publishers announce that the translation will be continued by Dr. H. J. Campbell, a gentleman who has served them in a similar capacity on more than one occasion. We would remind him that in respect to the general translation here set before him he has an example which he may well emulate, and express the hope that in the interests of English he will not allow the afore-mentioned extraordinary misuse of so commonplace a word to continue.

*Introduction to the Study of Fungi, their Organography, Classification, and Distribution for the use of Collectors.* By M. C. Cooke, M.A., LL.D., A.L.S. London: Adam & Charles Black.

Perhaps there is no group of plants more bewildering to the beginner than that of the Fungi. The number of known species is enormous, probably more than 40,000, and the frequent occurrence of polymorphism serves considerably to complicate the task of understanding their mutual relationships. Most of the existing text- and hand-books, even though estimable enough in their way, only produce a feeling of discouragement in the mind of the student, since they practically presuppose a degree of acquaintance with the general forms of the plants such as the reader in most cases does not possess. Hence he has either to greatly extend the scope of his reading by referring to original papers and figures, or, too commonly, he is contented with merely "getting up" special facts about an organism of whose general character he does not possess the remotest idea.

The great merit of Dr. Cooke's book lies in the fact that it contains a good deal of description of entire plants. Probably no one possesses a more extensive knowledge of the external characters of Fungi than the author, and his account is frequently enlivened with interesting details of habit and mode of life. He divides the work up into three main sections, Organography, Classification, and Distribution, and in an appendix gives useful hints as to collecting and preserving specimens.

The subject of Organography is dealt with rather from the standpoint of the systematist than from that of the comparative morphologist, and although this method of treatment is of necessity somewhat formal the student will find that it possesses practical advantages of its own, inasmuch as it provides a useful key with which to unlock the vast storehouse of facts buried in the more purely systematic treatises. Naturally, there are several points on which there will be differences of opinion between the author and his readers, and the records of certain alleged observations whose accuracy has never been admitted by persons most qualified to judge, might well have been omitted. Probably, too, many will dissent from the author's

opinion that in "Hymenomycetal Fungi really parasitic species are almost if not wholly unknown".

In the introduction to the section on Classification, a sketch of the Brefeldian system is given, though it is not adhered to by Dr. Cooke in the subsequent chapters. We notice that the author still apparently retains the opinion that Lichens are to be regarded as a group distinct from Fungi on the one hand and from Algæ on the other, but no new arguments are brought forward to support a position which has been long ago abandoned by nearly all botanists.

Speaking generally, one rather misses in these chapters the feeling a real and natural relationship existing between the different groups, and in some instances, as in the Uredineæ and Ustilagineæ, there is no effort made to trace it. This is especially disappointing when one recollects the brilliant expositions of Brefeld and his disciples on these questions. But notwithstanding this defect—for as such we must regard it—there is a great deal of valuable matter in these pages which will not be readily found elsewhere.

The concluding section on Distribution contains some interesting facts and generalisations, and statistics are given as to the relative proportions in which the various groups are scattered over the earth's surface. Thus amongst the Hymenomycetes it would appear that the more fleshy species are chiefly restricted to temperate and cold climates, whilst the tough and leathery forms are more especially characteristic of the tropical regions.

The book is well illustrated with figures, and the copious bibliographies appended to the chapters greatly add to the value of a work which deserves to be widely appreciated, not only by "Collectors," for whose use it was more especially designed, but by all who are interested in these lower orders of plants.

*Untersuchungen über die Stärkekörner.* By Dr. A. Meyer. Jena: Fischer, 1895.

This is one of the most important and valuable monographs that have recently been published, treating as it does of a somewhat small section of botanical research, which has nevertheless been the subject of much controversy, and has been the subject of investigation and speculation by many writers of great technical skill and critical acumen. It includes a summary of the researches of previous observers, and is enriched by independent observations of the author.

The starch grain seems at first sight to offer but little scope for speculation, yet upon its structure many points of great importance hang, which touch indeed the physical construction of protoplasm itself, and that of the many organised structures derived from it.

Dr. Meyer treats very carefully of the chemistry and physics of the starch grain as well as of its biological peculiarities. He considers it to consist of at least two different bodies, which he calls *amyloses*, both of which are crystalline, though one of them cannot be made form isolated crystals. Besides these other substances sometimes occur, which are also carbohydrates, but which are only found when the grains have been somewhat modified. It is a pity, perhaps, that he has selected the term *amylose* for these two constituents, as the termination *-ose* is so generally in use for sugars of various composition.

Dr. Meyer holds that the crystalline substance is in the form of fine needles, to which he gives the name *trichites*, and these are arranged in the grain in a radiating form, producing sphere-crystals, which are differently constituted in the several layers of the grain. The various physical properties which the latter presents are of course consistent with this view of its structure. The absorption of water which can so easily be brought about by the action of weak alkalis, etc., is discussed at some length. In the author's opinion it is brought about by the actual taking up of water by the crystalline trichites, and not by its intercalation between them; we have therefore a view opposed to the older theory of the micellæ put forward so many years ago by Naegeli. Dr. Meyer does not, however, deny the possibility of water being taken up and held by whatever lies between the crystals.

The fate of the starch grain after its formation, the action of diastase upon it, and the many possible reactions leading to the appearance of the various dextrins are also very fully discussed. This section of the work in particular will be of great interest to all students of vegetable physiology.

In the chapters devoted to the formation and growth of the starch grain some new views are advanced which will perhaps not be readily accepted by other workers in this field. He supports very strongly Schimper's views of the action of the leucoplast, but he states that the formation of the grain is always inside the corpuscle. Though this has long been recognised as happening sometimes, it seems difficult to reconcile certain cases of not uncommon occur-

rence with it. Dr. Meyer, however, thinks the observations on which the view of excretion of starch substance beyond the plastid is based are defective, and that a thin layer of the latter always extends around the incipient and growing grain, although it needs very careful staining by approved methods to make it visible. He further extends the amyloplastic powers of the leucoplast and the chlorophyll grain to certain chromoplasts.

Besides these researches based upon the normal grains as commonly seen, the book contains some very valuable information upon the changes which the starch grain shows under many varied conditions, the differences noticeable at different seasons, and the alterations in it in the various organs of many different plants, taken from a very wide range in the vegetable kingdom. Many ingenious experiments are narrated bearing on many of these points. They are, however, of less general interest and indeed of less importance than the points of origin, structure and fate which have already been alluded to.

The work will have a great fascination for many workers in the field of vegetable physiology. It is a pity that the language in which it is written will cause it to be less generally useful to the English reader than its importance warrants.

# APPENDIX II.

## CHEMICAL LITERATURE FOR FEBRUARY, 1896.

### Vol. i. No. 3. *American Journal of Science.* (March, 1896.)

*Gooch, F. A.*, and *Peirce, A. W.*, Method for the Separation of Selenium from Tellurium based upon the difference in volatility of the Bromides ( pp. 181-186). *Adams, F. D.*, and *Harrington, B. J.*, New Alkali Hornblende and a titaniferous Andradite from the Nepheline-Syenite of Dungannon, Hastings County, Ontario (pp. 210-219). *Penfield, S. L.*, and *Pratt, J. H.*, Occurrence of Thaumasite at West Paterson, New Jersey (pp. 229-234).

### Vol. xviii. No. 3. *Journal of the American Chemical Society.* (March, 1896.)

*Clarke, F. W.*, Third Annual Report of Committee on Atomic Weights. Results published during 1895 (pp. 197-214). *Johnson, S. W.*, Composition of Wood Gum (pp. 214-223). *Blair, A. A.*, Method for the Determination of Carbon in Steel (pp. 223-227). *Hopkins, C. G.*, A New Safety Distillation Tube for Rapid Work in Nitrogen Determinations (pp. 227-228). *Stone, G. C.*, Remarks on Mr. Auchy's Paper on the Volumetric Determination of Manganese (pp. 228-230). *Stone, G. C.*, Probable Production of Permanganate by Direct Combustion of Metallic Manganese (pp. 230-231). *Squibb, E. R.*, The Manufacture of Acetone and of Acetone-Chloroform from Acetic Acid (pp. 231-247). *Prescott, A. B.*, and *Baer, S. H.*, Pyridine Alkyl Hydroxides (pp. 247-251). *Andrews, L.*, On the Reduction of Sulphuric Acid by Copper, as a Function of the Temperature (pp. 251-254). *Wait, C. E.*, The Oxidation of Silver (pp. 254-259). *Wainwright, J. H.*, The Determination of the Solid Fat in Artificial Mixtures of Vegetable and Animal Fats and Oils (pp. 259-264). *Hazen, A.*, The Measurement of the Colours of Natural Waters (pp. 264-275). *Linton, L. A.*, Technical Analysis of Asphaltum, 2 (pp. 275-283).

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### Vol. xxi. No. 240. *The Analyst.* (March, 1896.)

*Blount, B.*, The Determination of Oxygen in Commercial Copper (pp. 57-62). *Cribb, C. H.*, A New Form of Carbonic Acid Apparatus (pp. 62-64).

### Vol. lxix. No. 400. *Journal of the Chemical Society.* (March, 1896.)

*Henderson, J.*, Action of Sugars on Ammoniacal Silver Nitrate (pp. 145-154). *Tilden, W. A.*, and *Barnett, R. E.*, The Molecular Weight and Formula of Phosphoric Anhydride and of Metaphosphoric Acid (pp. 154-161). *Bentley, W. H.*, *Haworth, E.*, and *Perkin, W. H., Jun.*, On $\gamma$-Phenoxy-Derivatives of Malonic Acid and Acetic Acid, and Various Compounds used in the Synthesis of these Acids (pp. 161-175). *Haworth, E.*, and *Perkin, W. H., Jun.*, Note on the Preparation of Glycol (pp. 175-177). *Luxmoore, C. M.*, The

Oximes of Benzaldehyde and their Derivatives (pp. 177-193). *Walker, J.*, and *Appleyard, J. R.*, Transformation of the Alkylammonium Cyanates into the corresponding Ureas (pp. 193-206). *Perkin, A. G.*, Luteolin, I. (pp. 206-212). *Hutchinson, A.*, and *Pollard, W.*, Lead Tetracetate and the Plumbic Salts (pp. 212-226). *Lewes, V. B.*, The Acetylene Theory of Luminosity (pp. 226-243).

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*Lovibond, J. W.*, The Effect of Lime Salts on Hop Infusions (pp. 71-75). *Reid, W. F.*, The Manufacture of Linoleum (pp. 75-79). *Irwin, W.*, The Effect of Heat on the Illuminating Power of Coal-Gas. Its Relation to the Theory of Flame (pp. 80-81). *Davis, G. E.*, Photography by the Röntgen Rays (p. 82). *Barnes, J.*, On the Estimation of Organic Matter by Means of Chromic Acid (pp. 82-84). *Archbutt, L.*, Note on the Ignition of Sawdust by Nitric Acid (pp. 84-85). *Archbutt, L.*, Note on an Experiment made to Determine the Pressure of Ether and some other Volatile Liquids in Closed Vessels (pp. 85-86). *Cohen, J. B.*, and *Russell, G. H.*, The Combustion of Coal and Gas in House Fires (pp. 86-90). *Mackey, McD. W.*, Apparatus for the Determination of the Relative Liability to Spontaneous Combustion of Oils spread on Cotton Wool (pp. 90-91). *Dott, D. B.*, Opium Assay (pp. 91-94). *Gane, E. H.*, The Determination of Caffeine in Tea (pp. 95-96). *Edwards, H. W.*, Bessemerizing Nickel Matte (pp. 96-99). *Peacock, S.*, American Phosphates in 1895 (p. 99).

Vol. xli. No. 250. *Philosophical Magazine and Journal of Science.* (March, 1896.)

*Nernst, W.*, and *Abegg, R.*, On the Freezing-points of Dilute Solutions (pp. 96-199).

Tome vii. *Annales de Chimie et de Physique.* (March, 1896.)

*Perreau, F.*, Etude expérimentale de la dispersion et de la réfraction des gaz (pp. 289-348). *Sabatier, P.*, et *Senderens, J. B.*, Recherches sur les oxydes de l'azote, oxyde azotique, oxyde azoteux, peroxyde d'azote (pp. 348-416). *Lescoeur, H.*, Recherches sur la dissociation des hydrates salins et des composés analogues (pp. 416-432).

Tomes xv.-xvi. No. 4. *Bulletin de la Société Chimique de Paris.* (20th February, 1896.)

*Guye, P. A.*, et *Chavanne, L.*, Etude sur la dissymétrie moléculaire : Recherches sur le pouvoir rotatoire des corps actifs homologues (pp. 177-195). *Tanret, C.*, Sur les modifications moléculaires et la multirotation des sucres (pp. 195-205). *Tassily*, Sur les iodures cristallisés de strontium et de calcium (pp. 205-206). *Varet, R.*, Recherches sur les cyanures de lithium, de magnésium, de cuivre (pp. 206-208). *Senderens, J. B.*, Nouvelles recherches sur les précipitations métalliques (pp. 208-221). *Iovitschtch, M.*, Contribution à la connaissance de la stéréoisomérie que présente l'éther isonitrosoacétylacétique (pp. 221-227). *Arth, G.*, Action de l'isocyanate de phényle sur l'acide $\gamma$-pimélique dérivé du menthol (pp. 227-229). *Reverdin, F.*, et *Kauffman, H.*, Sur quelques produits de substitution des carbonates et phosphates d'$\alpha$ et de $\beta$-naphtyle et sur la préparation du chloronaphtol $C_{10}H_6OH.\ Cl.\ (1.4)$ et du bromo naphtol $C_{10}H_6OH.\ Br\ (1.4)$ (pp. 229-235). *Bietrix, A.*, Sur une matière colorante dérivée de l'acide dibromogallique (pp. 235-236). *Lasne, H.*, Sur le dosage de l'aluminium dans les phosphates (iii.) (pp. 237-248). *Béchamp, A.*, Sur les altérations spontanées du lait et sur celles que la cuisson lui fait subir (pp. 248-272).

Tomes xv.-xvi. No. 5. (5th March, 1896.)

*Tassilly*, Appareil pour filtrer ou essorer les corps altérables à l'air (pp. 274-275). *Guye, P.*, et *Chavanne, L.*, Etude sur la dissymétrie moléculaire : Recherches sur le pouvoir rotatoire des corps actifs homologues (pp. 275-305). *Guye, P. A.*, et *Jordan, C.*, Formule simplifiée pour calculer les variations de densité des liquides avec la température (pp. 306-309). *Thomas, V.*, Action du peroxyde d'azote sur les sels halogénés d'etain (persels) (pp. 309-

315). *Racoura, A.*, Sur un nouvel acide du chrome, l'hydrate sulfochromique (pp. 315-321). *Trillat, A.*, Préparation des amines de la série grasse (pp. 321-322). *Perrir, G.*, Sur la propylnaphtylcétone β (pp. 322-324). *Haller, A.*, Synthèse partielle du camphre (pp. 324-327). *Rosenstiehl, A.*, Sur quelques réactions propres aux fuchsines et aux carbinols amidés (pp. 327-330).

Tome cxxii. No. 6. *Comptes Rendus hebdomadaires de l'Académie des Sciences.* (10th February, 1896.)

*Moissan, H.*, Etude du carbure d'uranium (pp. 274-280). *Haller, A.*, Sur la campholide, produit de réduction de l'anhydride camphorique (pp. 293-298). *Vigouroux*, Sur le siliciure de cuivre (pp. 318-320). *Besson, A.*, Sur le chlorobromure et le bromure de thionyle (pp. 320-322). *Granger, A.*, Sur un sulfophosphure d'étain cristallisé (pp. 322-323). *Tassilly*, Oxyiodures de zinc (pp. 323-325). *Brulle, R.*, Méthode pour déterminer la pureté des beurres au moyen de la densité (pp. 325-326).

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*Moissan, H.*, Préparation et propriétés du carbure de cérium (pp. 357-262). *Moissan, H.*, Sur le carbure de lithium (pp. 362-364). *Engel, R.*, et *Bernard, J.*, Sur un procédé rapide de dosage de l'arsenic (pp. 390-393). *Barbier, P.*, et *Bouveault, L.*, Synthése partielle de l'acide géranique ; constitution du lémonol et du lémonal (pp. 393-395). *Gassmann, C.*, Sur quelques dérivés de l'eugénol (pp. 395-398). *Schloesing, T., fils*, Sur la composition du grisou (pp. 398-401).

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Tome cxxii. No. 9. (2nd March, 1896.)

*Barbier, P.*, et *Bouveault*, Extraction du rhodinol, de l'essence de pélargonium et de l'essence de roses ; identité de ces deux alcools (pp. 529-531). *Combes, C.*, Sur la préparation du silicichloroforme, du silicibromoforme et sur quelques dérivés du triphényl-silico-protane (pp. 531-533). *Charon, E.*, Oxydation de l'aldéhyde crotonique (pp. 533-535).

16e Année. Tome iii. No. 4. *Journal de Pharmacie et de Chimie.* (15th February, 1896.)

*Cazeneuve, P.*, Note sur un nouvel outillage pour conserver les viandes par cuisson et élimination d'air (pp. 169-171). *Marie, T.*, Ethers glycériques des acides cérotique et mélissique (pp. 171-177). *Bourquelot, E.*, et *Bertrand*, G., Sur la coloration des tissus et du suc de certains champignons au contact de l'air (pp. 177-182). *Barthe, L.*, L'oxycyanure de mercure (pp. 182-184).

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*Kischner, N.*, Sur les amines et les hydrazines de la série polyméthylènique (pp. 521-553). *Selivanoff, T.*, Action des acides hypochloreux et hypobromeaux sur l'iodure de potassium (pp. 553-568). *Reformatsky, S.*, et *Pleskonosoff, W.*, Action du zinc sur l'éther bromoisobutyrique en présence de l'acétone (pp. 568-577). *Tichvinsky, M.*, Benzylrosinduline et benzylrosindulone (pp. 577-584). *Godlevsky, J.*, Oxydation du limonène; *Wagner, G.*, Méthodes pour déterminer la structure des olefines; *Wagner, G.*, Sur le formation simultanée des éthers nitreux et des combinaisons nitrées; *Ertchikoffsky*, Action de l'acide acètique sur le pinène eu présence du chlorure de zinc; *Ginsberg, A.*, La dehydratation du trioxyhexahydrocymène; *Petrenko-Kritchenko*, Action de la phenylhydrazine sur les éthers acetodicarboniques; *Charitschkoff, K.*, Sur la naphte de l'ile de Tcheleken; *Jakovkine, A.*, Sur la division d'une substance entre deux dissolvants; *Reformatsky, S.*, Formation de l'acide diméthylglutarique; *Daine*, Synthèse de l'acide phényloxypivaliquc; *Andres, L.*, Synthèse de l'acide éthylphényllactique; *Cuculesco, I.*, Sur l'acide diméthylisobutyllactique; *Zelinsky, D.*, Sur le triméthylhexamethylène (pp. 584-602).

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Band ccxxxiv. Heft 2. (22nd February, 1896.)

*Dragendorff, C.*, Beiträge zur gerichtlichen chemie (pp. 81-87). *Boettinger, C.*, Ueber einige Abkömmlinge des Acetessigäthers (pp. 87-91). *Boettinger, C.*, Ucber glyoxylsaures Natrium (pp. 91-92). *Virchow, H.*, Ueber Bau und Nervatur der Blattzähne und Blattspitzen (pp. 92-154). *Lutz, G.*, Ueber die oblito-schizogenen Secretbehälter der Mystaceen (pp. 154-158). *Boettinger, C.*, Ueber einige Abkömmlinge der Glykolsäure (pp. 158-160).

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Jahrgang xxix. No. 4. (9th March, 1896.)

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*Beckmann, E.*, und *Eickelberg, H.*, Zur Kenntniss der Menthone, Ueberführung in Thymol (pp. 418-421). *Ladenburg A.*, Zur Constitution des Tropins (pp. 421-422). *Ladenburg, A.*, Ueber das Isopipecolin (pp. 422-424). *Krafft, F.*, und *Lyons, R. E.*, Ueber Diphenylselenon $C_6H_5 . SeO_2 . C_6H_5$ (pp. 424-428). *Krafft, F.*, und *Kaschau, A.*, Ueber die Synthese der aromatischen Selenverbindungen vermittels Chloraluminium (pp. 428-435). *Krafft, F.*, und *Lyons, R. E.*, Ueber Thianthren (Diphenylendisulfid) $C_{12}H_8S_2$ und Selenanthren. I. (pp. 435-443). *Krafft, F.*, und *Kaschau, A.*, Ueber Thianthren und Selenanthren, $C_{12}$ $H_8$ $Se_2$. II. (pp. 443-446). *Bamberger, E.*, Experimentalbeiträge zur Chemie der Diazoverbindungen (pp. 446-474). *Van Erp, H.*, Ueber die Wirkung von schmelzendem Kali auf Methylnitramin und Dimethylnitramin (pp. 474-476). *Bondzynski, St. von*, Ueber das Cholesterin der menschlichen Faeces (pp. 476-478). *Euthyme* und *Klinienko, B.*, Ueber die Reaction der unterchlorigen Säure mit Chlorkobalt und Chlormangan (pp. 478-481). *Ciamician, G.*, und *Silber, P.*, Ueber die Alhaloïde der Granatwurzelrinde (pp. 481-490). *Ciamician, G.*, und *Silber, P.*, Ueber das n- Methyltroponin (pp. 490-494). *Wislicenus, H.*, Glatte Reduction der Nitrogruppe zur Hydroxylamingruppe (pp. 494-496). *Pschorr, R.*, Neue Synthese des Phenanthrens und seiner Derivate (pp. 496-501). *Meyenburg, F. v.*, Ueber die Einwirkung von Chlorkohlenoxyd auf Dimethyl- und Diäthyl- m- aminophenol (pp. 501-513). *Harries, C.*, und *Loth, G.*, Zur Constitution der 1- Phenyl- pyrazolone (pp. 513-521). *Harries, C.*, Ueber die Oxime der cyclischen Acetonbasen und das p- Aminotrimethylpiperidin (pp. 521-529). *Tiemann, F.*, und *Semmler, F. W.*, Ueber Pinonsäure (pp. 529-544). *Jnuger, E.*, und *Klages, A.*, Ueber Halogenderivate des Camphens und Hydrocamphens (pp. 544-547). *Lawrence, W. T.*, Ueber Verbindungen der Zucker mit dem Aethylen-, Trimethylen- und Benzylmercaptan (pp. 547-552). *Kraemer, G.*, und *Spilker, A.*, Ueber das Cyclopentadien im Steinkohlentheer, das Inden der Fettreihe (pp. 552-561).

Band liii. Nos. 2-3. *Journal für praktische Chemie.* (1st February, 1896.)

*Thudichum, J. L. W.*, Ueber das Phrenosin, ein unmittelbares Educt aus dem Gehirn und die Producte seiner Chemolyse mit Salpetersäure (pp. 49-91). *Zincke, T.*, und *Helmert, B.*, Zur Constitution der Azimide (pp. 91-100). *Zincke, T.*, Ueber die Umwandlung von Bromprotocatechusäure in eine Dibrom- o- Naphto- Chinoncarbonsäure (pp. 100-106). *Claus, A.*, und *Schnell, L.*, p- Nitrochinolin und p- Amidochinolin (pp. 106-127). *Knorr, L.*, Erwiderung auf die Abhandlung von R. von Rothenburg: Isomeriefälle in der Pyrazolreihe (Herm. L. Knorr zur Antwort) (pp. 127-132). *Walter, J.*, Druckrohr für Laboratoriumsversuche (pp. 132-139). *Schall, C.*, Ueber $\gamma$-Carbodiphenylimid (pp. 139-143). *Lottermoser, A.*, Zur Kenntniss der Einwirkung von Natruim auf aromatische Nitrile IV. (pp. 143-144).

Band xvi. Heft 10. *Monatshefte für Chemie und verwandte Thiele anderer Wissenschaften.* (31st January, 1896.)

*Glücksmann, C.*, Zur Bildung des Pinakolins aus Calcium iso-butyrat (pp. 897-906). *Herzig, J.*, Ueber Haematoxylin und Brasilin III. (pp. 906-919). *Kostanecki, St. v.*, und *Tambor, J.*, Ueber einen weiteren synthetischen Versuch in der Gentisinreihe (pp. 919-927).

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*Rosenheim, A.*, Ueber die Einwirkung organischer Metallsäuren auf organische Säuren (pp. 225-249). *Gooch, F. A.*, und *Peirce, A. W.*, Jodometrische Bestimmung der selenigen Saüre und der Selensäure (pp. 249-254). *Reizenstein, F.*, Ueber einige Metallsalze mit organischen Basen II. (pp. 254-264). *Flawitzky, F.*, Ueber eine Funktion, welche der Periodizität der Eigenschaften der chemischen Elemente entspricht (pp. 264-268). *Mawrow, F.*, und *Muthmann, W.*, Zur quantitativen Bestimmung und Scheidung des Kupfers (pp. 268-272). *Arctowski, H.*, Untersuchungen ueber die Löslichkeit beim Erstarrungspunkte der Lösungsmittel (pp. 727-28). *Hofmann, K. A.*, Ueber das Nitroprussidnatrium (pp. 278-288). *Hofmann, K. A.*, und *Wiede, O. F.*, Neue Darstellungsmethoden des Phenylesters der Eisentetranitrososulfosaüre (pp. 288-293).

Band xix. Heft 2. *Zeitschrift für Physikalische Chemie.* (25th February, 1896.)

*Linck, G.*, Beitrag zu den Beziehungen zwischen dem Krystall und seinem chemischen Bestand (pp. 193-201). *Ortloff, W.*, Beitrag zur Kenntnis eutropischer Reihen (pp. 201-228). *Bredig, G.*, Ueber Wärmeleitung und Ionenbewegung (pp. 228-233). *Wildermann, M.*, Experimenteller Beweis der van't Hoffschen Konstante, des Arrheniusschen Satzes, des Ostwaldschen Verdünnungsgesetzes, des Daltonschen Gesetzes u. s. w. in sehr verdünnten Lösungen (pp. 233-251). *Zanninovich-Tessarin, H.*, Elektrolytische Dissociation der Lösungen in Ameisensäure (pp. 251-261). *Le Blanc, M.*, und *Rohland, P.*, Ueber den Einfluss, welchen die elektrolytische Dissociation, der Wechsel des Aggregatzustandes und des Lösungsmittels auf das Lichtbrechungsvermögen einiger Stoffe Ausüben (pp. 261-287). *Hertlein, H.*, Beiträge zur Kenntnis der Polythionate (pp. 287-318). *Van, Laar, J. J.*, Zur Antwort an Herrn Prof. W. Nernst (pp. 318-323). *Rodger, J. W.*, und *Watson, W.*, Ueber die magnetische Drehung der Polarisationsebene des Lichtes in Flüssigkeiten. I., Teil: Schwefelkohlenstoff und Wasser (pp. 323-380).

# APPENDIX I.

## NOTICES OF BOOKS.

*The Cambridge Natural History*, vol. v., "Peripatus," by Adam Sedgwick; "Myriapods," by F. G. Sinclair; "Insects," by D. Sharp. 8vo, pp. xi. and 584. London: Macmillan & Co., 1895.

We were told in the prospectus that "*The Cambridge Natural History* is intended, in the first instance, for those who have had no special scientific training, and who are not necessarily acquainted with scientific language". Mr. Sedgwick's chapter on Peripatus in the present volume gives a very curious interpretation to this intention. This chapter is, to a large extent, written in language which is nothing if not scientific, and which, however clear and intelligible to the well-versed student, must, at times, be completely mystifying to the untutored. Except in a few pages, where in simple words he describes the living Peripatus and its habits and external features, the style of his writing is much better suited to a scientific monograph or a text-book of comparative zoology. Here is a sample of it, taken from the section on development. "The segmentation is peculiar, and leads to the formation of a solid gastrula, composed of a cortex of ectodermal nuclei surrounding a central endodermal mass, which consists of a much-vacuolated tissue with some irregularly shaped nuclei. The endoderm mass is exposed at one point—the blastopore (gastrula mouth). The central vacuoles of the endoderm now unite and form the enteron of the embryo." The anatomy of Peripatus is somewhat similarly treated of in another section. These two sections, the most important in the chapter, will, no doubt, give pleasure to those readers who already possess some knowledge of the subjects, and to those who do not they may prove an incentive to go through a course of special training. Some excellent figures are scattered through the chapter, and there is a map to show the geographical distribution of the genus. A list of species, with their localities, is appended, and some of the species are briefly characterised. This list, or synopsis as it is called, may be useful to some readers, but for the systematic student it will have little value, as Mr. Sedgwick has apparently not taken the trouble to bring it up to date, and it differs in no essential respect, so far as we can judge, from one which he published with his monograph several years ago.

Mr. Sinclair tells us in his chapter on the Myriapods that he "only aims at giving an outline sketch of the group that shall be intelligible to the general reader who has not made a special study of such matters". In this aim he has, to some extent, succeeded very well. But this is not all that we were led to expect. For did not the prospectus also say that "an attempt would be made, not only to combine popular treatment with the latest results of scientific research, but to make the volumes useful to those who may be regarded as serious students of the various subjects"? We do not wish to imply that Mr. Sinclair has not made such an attempt. Evidence of it is in fact to be found in those parts of the chapter in which he writes on subjects with which his own researches have been particularly associated, but we find very few traces of it elsewhere. His general introduction, in which he speaks of the habits of Myriapods, is good, and his account of the structure and development of some of the groups leaves nothing to be desired. But when we turn to his treatment of classification and his account of some of the smaller but not less interesting orders there is a different tale to tell. Mr. Sinclair is apparently unaware that the whole subject of the classification of Myriapods has in recent years been completely revised, and his own attempt at classification is a mere revival of the antiquated system of Koch, with the addition of the two orders Symphyla and Pauropoda. Pocock and Kingsley, to mention two only of the chief authorities, will scarcely feel flattered to find that there is not the slightest reference to their views published several years ago, and not yet so far as we know disputed, that the so-called Myriapods do not constitute a single homogeneous class but consist of least two very distinct groups. We do not feel competent to follow Mr. Sinclair through his detailed accounts of the different orders, but if his treatment of the Symphyla may be taken as a sample of the rest, his performance in this respect is very poor indeed. Scolopendrella, the sole genus of this order, shows certain points of resemblance to the Thysanura, and is by some considered to be the living type which comes nearest to the ancestral forms of both Myriapods and Insects. Mr. Sinclair very properly calls

attention to the special interest which thus attaches to it, but how does he proceed to satisfy the desire for more detailed information? He gives no figure of the genus, and as to the number of species or as to the habits or habitat of any one of them, although two species are, we believe, common enough in this country, he says not a word. He points out it is true how Scolopendrella differs from Campodea, but in what respects it exhibits "a great resemblance to the Thysanura" he forgets to mention. We are told in one place that it has a pair of legs to each segment of the body, and in another that the smaller segments do not bear legs. The caudal appendages are described as hook-like, but why they should appear so to Mr. Sinclair and not to others we need not stop to inquire. The genital opening of Scolopendrella, he tells us, is on the last segment of the body, though he gives no reason for refusing to accept the statements of Ryder, Grassi, Haase and others, who tell us on the contrary that this opening is on the fourth segment. There is of course no reference to the remarkable coxal spurs and saccules of Scolopendrella; such matters being perhaps considered outside the interest of the general reader. As the volume is mainly entomological and intended as well for serious students, a fuller treatment of these structures would not we think be altogether out of place. In other parts of the chapter we notice a few inaccuracies which might with ordinary care have been avoided. Thus we read on p. 59 that "the generative system of Chilopoda differs chiefly in the opening of the genital apparatus at the end of the body instead of in the third segment; though this difference only separates the order from the Chilognatha and not from the other orders". The natural inference from this statement is that in all the other orders the genital opening is at the end of the body, but this is true only of the Schizotarsia. Again on p. 43 it is stated that the genital organs of the Chilognatha open on "one of the anterior rings of the posterior part of the body, usually the seventh". But this palpable slip is corrected in another place. Mr. Sinclair's references to Cuvier on page 77 we must also attribute to carelessness, for he could scarcely be so ill acquainted with the history of his subject as not to know that it was Latreille and not the great anatomist "who united the Myriopods with the Insects, making them the first order and the Thysanura the second," and who was thus the first to "claim a close relationship" between the two groups.

Dr. Sharp's Chapters on the Insects fill more than 500 of the whole 584 pages in the volume. They cover only a part of his subject, which is to be continued in another volume entirely devoted to the purpose. When we consider the vast extent of entomological literature and the number and variety of workers who have been engaged on this branch of science, it must be admitted that to write such an account of Insects as shall embrace all the most valuable and most generally interesting facts and shall at the same time be free from serious errors is by no means an easy task, and requires the exercise of considerable knowledge and judgment. The success with which Dr. Sharp has so far accomplished this task is not surprising to those who know him, but is not the less a matter for congratulation to himself as well as to his readers. His work is in most respects brought well up to date, and puts the reader in touch with nearly all the latest researches in every branch of Entomology, while for the student who wishes to follow up any particular subject copious references to original memoirs are supplied. Dr. Sharp is generally very guarded, almost too guarded in his statements, appearing throughout as the impartial recorder rather than as the exponent of any particular views, and seldom giving expression to his own opinion even on matters on which it might be expected to carry much weight. He refers for example without any comment to the suggestion that the elytra of beetles are homologous with the tegulæ and not with the anterior wings of other insects, though we have good reason to know that he himself holds the opposite view. Occasionally, however, he betrays some indication of his leanings. Thus on the subject of insect-vision he seems to hold with those who believe that insects perceive only "the lights, shades, and movements of the external world," and can distinguish neither form nor colour. He does not state this explicitly, but such is the inference we draw from the few remarks he makes on this interesting subject. We notice too that in describing the structure of the compound eye he omits all reference to the view long ago expressed by Straus-Durckheim and recently revived by Van Patten, that the crystalline cones are really percipient and not merely dioptric elements of the eye. Dr. Sharp's writing is generally very clear, but there are one or two places in which he leaves us in some doubt as to the drift of his remarks. From what he says on p. 89 he seems to admit the probability that in different insects the head is composed of a different number of primary segments, from three to six or even possibly seven, and that the "thorax" also may in some insects be composed of six and in others of three primary segments. Again in a footnote on p. 91, he speaks of the wings as "appendages" which "differ but little in their nature from legs". If he really holds the remarkable views which seem to be implied by his words in both these cases, we should like him to have stated them a little more clearly. One

or two other points in his account of the structure of insects call for some slight notice. "Comparison," he says, "suggests that the hypoglottis of Coleoptera may possibly represent the piece corresponding to the mentum of Orthopterists, the so-called mentum of beetles being in that case the sub-mentum of Orthopterists". This suggestion agrees very well with the statements appearing in so many text-books of comparative anatomy, in which the sub-mentum of Orthoptera is treated as part of the lower lip, and it seems to be supported by the figure which Dr. Sharp gives of the mouth parts of Locusta. But this figure is we fancy not altogether accurate. Comparison really seems to show that entomologists generally are right in regarding the sub-mentum of Orthoptera as part of the head, and homologous with the sub-mentum of beetles. This at least is the view which Waterhouse has taken[1] after instituting a series of very careful comparisons. Entomologists, however, are not always consistent in their use of anatomical terms, and we fancy we see an illustration of the fact in Dr. Sharp's use of the term clypeus. For what he figures and describes as the clypeus in the case of the cricket's head, does not seem to correspond with that part of the head of the cockroach which he denotes by the same name. This is, however, a minor point, and in a writer of less general accuracy than Dr. Sharp would probably escape notice altogether. There are a few omissions which had they been supplied would have added to the value of some of the chapters. In his general account of the embryonic development of insects, he says nothing about the post-oral origin of the antennae or of the appearance of leg-rudiments on the abdomen. His account of the Thysanura, remarkably full in other respects, is deficient in information about the interesting character of the mouth-parts, which is all the more to be regretted as it has so much bearing on the suggestion, to which in another place he refers, that the hypopharynx or rather the lobes at its base represent an additional pair of mouth-appendages. The least satisfactory part of Dr. Sharp's work is perhaps that in which he deals with the general classification of insects. Here he discusses the different systems proposed, and shows what every one is ready to admit that none of them is perfect, while at the same time he endeavours to excuse himself for reverting to one of the oldest and least natural of all. With his treatment of the different orders, which in this volume include the Aptera, Orthoptera, Neuroptera and a portion of the Hymenoptera we have no fault to find. He really seems to discount his own views on classification by the care with which he points out the close affinities between many of the groups of Pseudo-neuroptera and the true Orthoptera. The work is excellently illustrated, and besides being full of interest for the general reader will prove extremely useful to the student. It promises to be when completed the best modern text-book of Entomology in the English language.

*Grundzüge der Marinen Tiergeographie; Anleitung zur Untersuchung der geographischen Verbreitung Mariner Tiere, mit besonderer Berücksichtigung der Dekapodenkrebse.* Von Dr. Arnold E. Ortmann in Princeton, N.J., U.S.A. Mit 1 Karte. Jena, 1896. 96 pp.

Dr. Arnold Ortmann's clever essay is principally concerned with the distribution of marine animals, objects with which his own studies have made him especially familiar. But he here only uses them to illustrate the general principles which he desires to commend to the student of distribution.

In a useful historical summary he sets forth the successive attempts that have been made to explain or to describe the real or supposed distribution of animals now living on the globe. At the outset it was not unnatural to fancy that the range of animal groups would be determined by the zones of temperature. A striking personality like the polar bear for instance is not met with in the tropics, nor are there any arctic or antarctic monkeys, and in the ocean reef-corals will not support a temperature below 66° F. For the division of the zones into regions and subregions, various zoologists selected the range of some particular species or group with which they happened themselves to be best acquainted. But the typical species sometimes turns out to represent nothing but itself, and in the mapping out of provinces and districts there is no security that the boundaries will apply to any animals but those on which they were empirically based. To Andrew Murray is awarded the commendation that as early as 1866 he "inquires into causes for the existing condition of things, and finds them in the geological development of the earth, in the changing distribution of land and water, and at the same time lays stress on the importance of barriers and the limits of range". Thus was he an important forerunner of Wallace, who first effectively established the fundamental principles that the distribution of life is dependent on the geological history of the earth's surface, on

[1] *The Labium and Sub-mentum in certain Mandibulate Insects*, by C. O. Waterhouse.

limits of range, on means of dispersal, and that the two latter influences are different for different animals.

Dr. Ortmann considers that the pecularities in the conditions of existence which affect animal life may conveniently be grouped under three headings, as those which have to do with light, medium and substratum. Without light there is no vegetation. Without vegetation there is no food which animals can assimilate. According to the medium in which they live they must be fitted for air-breathing or water-breathing. The substratum may be dry land or ocean floor, but those animals which are dependent upon either must have the locomotive apparatus by which they obtain their food adapted accordingly. From these considerations Dr. Ortmann divides the globe into five principal life-areas. The medium distinguishes the land-area, of which the occupants are air-breathers, from all the rest. The abyssal area is set apart from all by the absence of light. The pelagic area stands alone in having occupants independent of any solid resting-place. The fluvial or fresh water area carries its characteristic in its name, and there remains the littoral area, not so sharply marked off as the others, but perhaps the most important of all, if from its teeming bosom the thronging forms of life have felt and found their way into all the other areas, spreading over the high seas, colonising the profoundest abysses, threading their course up estuaries and rivers, climbing the terraces of the land and taking wing beyond the clouds.

By paying regard to climatic and topographical relations, Dr. Ortmann finds himself able to subdivide the littoral and the pelagic areas into regions and subregions. In each there is an Arctic, an Antarctic, and an Indo-pacific region. In the pelagic area there is also an Atlantic region, while in the littoral there are three additional regions, a West-American, an East-American, and a West-African. In the last a Mediterranean subregion is distinguished from a Guinea subregion, and there are similar and further subdivisions suggested in some of the other areas. Only the abyssal area is spoken of as "without differentiation into regions and subregions". As distinguished from the Continental, Freshwater, and Littoral areas, the author maintains that "in the Abyssal and Pelagic areas the continuity is complete, in no part of the earth are special portions of these two topographically separated from others, but everywhere they stand in direct communication". This mode of viewing the abyssal area seems to be of very doubtful validity. There are submarine mountains, submarine lakes, warm currents and cold currents functioning as submarine rivers, which must operate as climatic and topographical barriers as forcibly in the unlighted marine abysses as they do in the realms of daylight. Considering, too, the intimate dependence of animal life upon the available food, it would be strange indeed if no regions and subregions were marked by the varying character of the ocean floor, with its diatom ooze and radiolarian ooze and globigerina ooze, and other distinctive coatings. Were the deep sea in fact an uninterrupted uniform expanse, it might be expected, and at one time was expected, to have a fauna common to the whole of it. But of this there is at least no striking evidence, and Dr. John Murray of *The Challenger* adduces some evidence which is rather striking in the contrary direction. Thus at a station in mid-equatorial Atlantic, in 1850 fathoms, 38 species were obtained. At a station in mid-equatorial Pacific, in 2425 fathoms, 29 species were obtained. Both were on globigerina ooze. Only one species was common to the two localities, and that one the little *Discina Atlantica*, belonging to a genus which ranges from the Cambrian to the present time.

In the vast area of the subject there are many regions and subregions of discussion into which this short notice cannot follow Dr. Ortmann. All that he has to say, whether it commands assent or otherwise, will be found worthy of attention. He brings very clearly into view the merits and occasionally the demerits of his predecessors. He shows how much we have still to learn, what points of vantage have been attained, in what direction the line of advance should be followed with most hope of success. The zoologist can scarcely peruse this memoir without finding that his own scientific studies from one side or another are closely connected with the complex problem of the distribution of animals.

# APPENDIX II.

## CHEMICAL LITERATURE FOR MARCH, 1896.

Vol. i. No. 4. *American Journal of Science.* (April, 1896.)

*Wolff, J. E.*, Occurrence of Theralite in Costa Rica, Central America (pp. 271-273). *Merrill, G. P.*, Occurrence of Free Gold in Granite (pp. 309-312).

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Vol. xviii. No. 3. *American Chemical Journal.* (March, 1896.)

*Orndorff, W. R*,, and *Terrasse, G. L.*, The Molecular Weight of Sulphur (pp. 173-207). *Mabery, C. F.*, On the Determination of Sulphur in Illuminating Gas and in Coal (pp. 207-215). *Mabery, C. F.*, and *Dunn, O. C.*, Chemistry of the Berea Grit Petroleum (pp. 215-236). *Morse, H. N.*, and *Chambers, A. D.*, A Method for the Standardisation of Potassium Permanganate and Sulphuric Acid (pp. 236-238). *Jackson, C. L.*, and *Gallivan, F. B.*, Some Derivatives of Unsymmetrical Tribrombenzol (pp. 238-252).

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*Allen, A. H.*, Note on the Titration of Quinine (pp. 85-87). *Allen, A. H.*, Note on the Preparation of Pure Hydrochloric Acid (pp. 87-88). *Richmond, H. D.*, The Composition of Milk and Milk Products (pp. 88-92). *Richmond, H. D.*, and *Boseley, L. K.*, Further Notes on the Detection of Formalin (pp. 92-94). *Hehner, O.*, The Detection of Formalin (pp. 94-99). *Shepherd, H. H. B.*, Official Methods for the Analysis of Fertilisers, issued by the German Manure Manufacturers' Association (pp. 99-102).

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*Humphreys, W. J.*, Solution and Diffusion of certain Metals in Mercury (pp. 243-253). *Bone W. A.*, and *Perkin, W. H., Jun.*, The Symmetrical Dimethylsuccinic Acids (pp. 253-268). *Bone, W. A.*, and *Perkin, W. H., Jun.*, Note on the αα-Dimethylglutaric Acids (pp. 268-270). *Bentley, W. H.*, *Perkin, W. H., Jun.*, and *Thorpe, J. F.*, *Cis-* and *trans-*Methylisopropylsuccinic Acid (pp. 270-287). *Wood, T. B.*, Available Potash and Phosphoric Acids in Soils (pp. 287-292).

Vol. xv. No. 3. *Journal of the Society of Chemical Industry.* (31st March, 1896.)

*Schack-Sommer, G.*, Sorghum Sugar Manufacture in Spain (p. 155). *Saniter, E. H.*, The Analysis of Chrome Ore and Ferro-Chromium (pp. 155-158). *Grossmann, J.*, Recent Developments in the Manufacture of Chlorates (pp. 158-161). *Macadam, S.*, On Destructors for Consuming Town Refuse—their Requirements, Defects, and Modes of Improvement (pp. 162-169). *Clowes, F.*, The Estimation of Oxygen by Alkaline Solution of Pyrogallol (p. 170). *Caven, R. M.*, Some Properties of Ferric Phosphate (Discussion) (pp. 170-171). *Richardson, F. W.*, and *Aykroyd H. E.*, Sulphides, Sulphites, Thiosul-

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*Lord Raleigh*, On some Physical Properties of Argon and Helium (pp. 198-208). *Tilden, W. A.*, An Attempt to Determine the Condition in which Helium and the Associated Gases exist in Minerals (218-224). *Schunck, E.*, and *Marchlewski, L.*, Contributions to the Chemistry of Chlorophyll VII., Phylloporphyrin and Hæmatoporphyrin—a Comparison (pp. 233-236). *Dunstan, W. R.*, and *Boole, L. E.*, An Enquiry into the Nature of the Vesicating Constituent of Croton Oil (pp. 237-249).

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*Tassilly*, Oxyiodures de Zinc (pp. 345-347). *Urbain, G.*, Contribution à l'étude du thorium (pp. 347-349). *Tanret, C.*, Sur les modifications moléculaires et la multirotation des sucres (pp. 349-361). *Secretant*, Ethers phosphoriques des phénols polyatomiques (pp. 361-364). *Reychler, A.*, Sur le chlorure de géranyle (pp. 364-366). *Reychler, A.*, Études sur les terpènes (pp. 366-376).

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*Carnot, A.*, Analyse, par les procédés volumétriques, d'un mélange de chlorures, d'hypochlorites et de chlorates (pp. 393-397). *Carnot, A.*, Analyse d'un mélange de chlorures, de chlorates et de perchlorates (pp. 397-399). *Varet, R.*, Loi des doubles décompositions entre le cyanure de mercure et les sels des métaux alcalins et alcalind-terreux (pp. 399-400). *Fournier, H.*, Sur les carbures diéthyléniques (pp. 400-404). *Biétrix, A.*, Action de la nitroso-diméthylaniline sur quelques dérivés bromés de l'acide gallique (pp. 404-409). *Genvresse, P.*, Contribution à l'étude des disulfures aromatiques (pp. 409-426). *Bèchamp, A.*, Sur les altérations spontanées du lait et sur celles que la cuisson lui fait subir (pp. 426-455). *Urbain, G.*, Sur la réaction de Schiff (pp. 455-456). *Violette, C.*, Azurage des farines par le bleu d'aniline. Comment on peut le reconnaître (p. 456).

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*Gautier, A.*, et *Hélier, H.*, Sur quelques conditions qui règlent les combinaisons gazeuses. Union de l'oxygène à l'hydrogène aux basses températures (pp. 566-573). *Moissan, H.*, et *Etard*, Sur les carbures d'yttrium et de thorium (pp. 573-578). *Thomas, V.*, Action du peroxyde d'azote et de l'air sur le chlorure de bismuth (pp. 611-613). *Coquillion, J.*, Sur les modifications apportées au grisoumètre et sur la limite d'approximation qu'il peut donner (pp. 613-615). *Schloesing, T.*, et *Richard, J.*, Recherches de l'argon dans les gaz de la vessie natatoire des Poissons et des Physalies (pp. 615-617). *Rivals, P.*, Étude thermochimique des amides et des sels ammoniacaux de quelques acides chlorés (pp. 617-619). *Scheurer-Kestner*, Sur la détermination de l'acidité des produits pyroligneux (pp. 619-621). *Ferrand*, Sur une nouvelle série de sulfophosphures : les thiophosphates (pp. 621-622). *Combes, C.*, Sur quelques dérivés du triphénylsilicoprotane (pp. 622-624). *Bouchardat, G.*, et *Tardy*, Sur l'essence d'anis de Russie (pp. 624-626).

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*Bogorodsky, A.*, Sur les cryohydrates (pp. 1-10). *Chaternikoff, M.*, et *Setchenoff, J.*, Appareil pour analyse des gaz (pp. 10-17). *Tichvinsky, M.*, Sur les relations des safranines et des indulines (pp. 17-24). *Reformatsky, S.*, Action du zinc et de l'ether bromoisobutyrique sur l'aldehyde isobutyrique (pp. 24-40). *Barilowitsch, A.*, Sur l'acide diisopropyloxalique (pp. 40-47). *Ostropiatoff, P.*, Action de la potasse sur l'acétonechloroforme (pp. 47-56). *Wagner. G.*, Sur la structure des terpènes et composés congenères (pp. 56-109). *Markownikoff, W.*, La source de Narsane ; *Menschutkin, N.*, Sur les sels des amides ; *Saposchnicoff, A.*, Sur les propriétés des solutions aqueuses de l'acétone ; *Melikoff, P.*, Sur le météorite de Zmen ; *Wagner, G.*, Sur le dosage de l'agrostemme dans la farine ; *Speransky, A.*, Sur le rhodanure de chrome ; *Melikoff, P.*, Sur les conditions de la formation de la soude naturelle (pp. 109-116).

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Zur Geschichte des Proteacins (pp. 314-317). *Hesse, O.*, Ueber den Zuckerbusch (pp. 317-321). *Zincke, T.*, Ueber die Einwirkung von Chlor auf Oxychinoline (p. 321). *Zincke, T.*, und *Winzheimer, E.*, Ueber Chloroxy-α-chinolin-chinon und dessen Umwandlungsproducte Hydrinden-, Inden- und Acetophenonderivate der Pyridenreihe, (pp. 321-359). *Zincke, T.*, und *Wiederhold, K.*,. Ueber Dichlor- β-chinolinchinon und dessen Umwandlungsproducte (pp. 359-382).

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*Bamberger, E.*, Zur Kenntniss normaler Diazometallsalze (pp. 564-577). *Veley, V. H.*, Die Reactionsunfähigkeit alkalischer Erden gegen Chlorwasserstoffgas (pp. 577-581). *Fischer, E.*, und *Bromberg, O.*, Ueber eine neue Pentonsäure und Pentose (pp. 581-586). *Richter, M. M.*, Ein Beitrag zur Nomenclatur (pp. 586-608). *Bamberger, E.*, Notiz, betreffend die Ionenzahl der diazosulfonsauren Salze (pp. 608-610). *Scholtz, M.*, Zur Kenntniss der Semicarbazone (pp. 610-613). *Scholtz, M.*, Ueber einige Derivate des Zimmtaldehyds (pp. 613-615). *Staedel, W.*, Synthese des Diphenylketons und seiner Derivate (pp. 615-616). *Zelinsky, N.*, Zur Kenntniss der stereoisomeren Dimethyltricarballylsäuren (pp. 616-618). *Darmstaedter, L.*, und *Liffschütz, J.*, Beiträge zur Kenntniss der Zusammensetzung des Wollfetts (pp. 618-622). *Rupe, H.*, und *Heberlein, G.*, Ueber unsymmetrische α-Phenylhydrazinderivate (pp. 622-623). *Pinnow, J.*, und *Sämann, C.*, Ueber Derivate des o- Amido-benzonitrils (pp. 623-633). *Reissert, A.*, Ueber die Einwirkung von o- und p- Nitrobenzylchlorid auf Natriummalonsäureester und einige analoge Verbindungen (pp. 633-639). *Reissert, A.*, Umwandlungen des o- Nitrobenzylmalonsäureesters I. Alkalische Verseifung, Synthese neuer Indolabkömmlinge (pp. 639-665). *Reissert, A.*, Umwandlungen des o- Nitrobenzylmalonsäureesters II. Reduction, Bildung von Chinolinderivaten (pp. 665-667). *Traube, W.*, Ueber einen neuen Weg zur Gewinnung aliphatischer Diazoverbindungen (pp. 667-670). *Traube, W.*, und *Longinescu, G. G.*, Ueber Hydrazinosäuren (pp. 670-676). *Hell, C.*, und *Portmann, B.*, Ueber die Einwirkung des Natriumäthylats auf Aethylisoeugenoldibromid (pp. 676-682). *Hell, C.*, und *Hollenberg, A.*, Ueber die Einwirkung von Natriumäthylat auf Anetholdibromid und Monobromanetholdibromid (pp. 682-691). *Tiemann, F.*, und *Schmidt, R.*, Ueber Homolinalool (pp. 691-695). *Reychler, A.*, Ueber die Einwirkung von Trichloressigsäure auf Terpene (pp. 695-697). *Reychler, A.*, Ueben Isobornylchlorid und Camphenchlorhydrat (pp. 697-699). *Hantzsch, A.*, und *Schultze, W.*, Ueber Isomerie beim Phenylnitromethan (pp. 699-703). *Knueppel, C. A.*, Ueber eine Verbesserung des Skraupschen Verfahrens zur Darstellung von Chinolin und Chinolinderivaten (pp. 703-710), *Michaelis, A.*, und *Luxembourg, K.*, Ueber anorganische Derivate der secundären aliphatischen Amine (pp. 710-716). *Michaelis, A.*, und *Silberstein, E.*, Ueber Oxyphosphazoverbindungen (pp. 716-729). *Zelinsky, N.*, und *Generosow, A.*, Untersuchungen in der Hexamethylenreihe IV. Abhandlung Synthese von Heptanaphten (pp. 729-734). *Klinger, H.*, und *Lonnes, C.*, Ueber die Einwirkung von Schwefelsäure auf Benzilsäure (pp. 734-742). *Wislicenus, W.*, Ueber das Verhalten alkalischer Lösungen des Formylphenylessigesters gegen Säuren (pp. 742-743). *Gerilowski, D.*, und *Hantzsch, A.*, Weiteres ueber die stereoisomeren Salze aus Diazosulfanilsäure (pp. 743-756).

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Derivate des m- nitro- o- Amidobenzamids und m- nitro- o- Amidobenzhydrazids (pp. 210-225). *Bertram, J.*, und *Gildemeister, E.*, Ueber Geraniol und Rhodinol (pp. 225-237). *Hesse, A.*, Ueber die vermeintliche Identität von Reuniol, Rhodinol und Geraniol (pp. 237-241). *Vaubel. W.*, Der Benzolkern VI. (pp. 241-246). *Herfeldt, G.*, Zur Kentniss der Kyanalkine, ins besondire des Kyanbenzylins (pp. 246-250). *Brüggemann, F.*, Ueber Derivate des Veratrols (pp. 250-255). *Liebermann, C.*, Herrn. Michael zur Erwiderung (pp. 255-256).

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*Giustiniani, E.*, Sopra alcuni costituenti dell'ortica (pp 1-7). *Angeli, A.*, e *Rimine, E.*, Sopra il nitrosito dell'isosafrolo (pp. 7-13). *Cannizzaro, S.*, e *Andreocci, A.*, Sulla costituzione del dimetilnaftol proveniente dalla scomposizione degli acidi santonosi (pp. 13-35). *Namias, R.*, Considerazioni fotochimiche e termofotochimiche (pp. 35-52). *Baltiano, L.*, Brevi osservazioni sulla nota dei signori Fr. Mahla e Ferd. Tiemann: Zum Abbau der Camphersäure (pp. 52-61). *Garelli, F.*, Sopra alcune soluzioni solide formate da sostanze non isomorfe (pp. 61-88).

# APPENDIX I.

## NOTICES OF BOOKS.

*Lehrbuch der Entwicklungsgeschichte des Menschen und der Wirbelthiere.* Von Prof. Dr. O. Hertwig (Fünfte theilweise umgearbeitete Auflage). Jena: Gustav Fischer, 1896.

The fact that this admirable work has entered upon a fifth edition in its ninth year is sufficient testimony to its success. The plan of the book remains unaltered, but the whole has been brought up to date. The sections dealing with the Structure of the Chorion, the Development of the Intermediate Germ Layer in Reptiles and Mammals, and the Genesis of the Cellular Elements of the Blood, are conspicuous among those which have received attention, and the recent work of Keibel, Will, von Kupffer, Kundrat and Engelmann, Leopold, Minot, and others, has been largely laid under obligation. This fifth edition is, however, most noteworthy for the fuller treatment of cytological topics, there having been added sections dealing with the *rôle* of the Centrosome in Fertilisation and the Reduction Division, and a short chapter of eight pages upon the "Mosaic Theory" of Roux and recent experimental work which bears upon it. Twenty-two illustrations have been added; but, seeing how great has been the success of this work, we had wished for the replacement in late editions of the hundred and one well-worn illustrations which nauseate us by their reappearance in text-book after text-book. Rathke's time-honoured diagrams of the development of the aortic arches are once more reproduced in full, but the work of Boas, Hochstetter, and Zimmermann, upon the pre-pulmonary arch, which has undermined them, is mentioned only in small type. This is but one of several instances in which recent work of a far-reaching order is insufficiently transcribed, and in some cases the incorporation in the "Literature" of titles of important papers has been considered sufficient recognition of their authors' work. We regret the introduction of Fol's *Quadrille des Centres* (notwithstanding the mention of the adverse results obtained by Boveri, Wilson, and Mathews) and of the would-be corroborative statements of Guignard. This and certain other very debatable topics might well have been left aside, in preference say for a fitting recognition of substantial observations such as Mitsukuri's upon the Mesoderm and Cœlom of the Chelonia and Haacke's and Giacomini's upon the allantoir placenta of the Lacertilia. The scanty recognition of the Invertebrata has been regarded as an objection to this work, it having been looked upon as ignoring the great middle series which lie between man and the higher animals and the lower organisms. This opinion appears to us to have arisen from a misconception of the author's aims, and it is certainly less justified of the present edition than any of its predecessors. The book deals professedly with the broader aspects of the organology of the vertebrata and with cytological questions which largely border on the physiological and the study of first principles; and in these associations the lower animals appear to us to have received ample consideration at the author's hands. His work is emphatically one for medical students, and as meeting their demands it appears to us unequalled. It is now well-established, and if the author would give us an edition in which illustrations, new and numerous, should be of the same excellence as the text, he would confer a boon on medical education.

*Evolution in Art: as Illustrated by Life-Histories of Designs.* By Alfred C. Haddon, Professor of Zoology, Royal College of Science, Dublin. With 8 plates and 130 figures in the text. The Contemporary Science Series. London: Walter Scott, Limited, 1895.

There is no training for a young biologist equal to a sojourn amongst the strange plants and animals of a tropical region. Darwin in the *Challenger*, Huxley in the *Rattlesnake*, received such a training and acquired the methods and material that made their reputations. It is an education that gives the mind of the scientific worker a broad bent, and makes many subjects have an interest for him. Before Professor Haddon made his journey to British New Guinea and the adjacent coasts he was known as a zoologist, a geologist, and an embryologist, but since then he has become better known as an anthropologist. It was his hap to land in a

region inhabited by mixed races of uncertain and puzzling origin, with strange customs and habits, and, above all, with a crudely rich and elaborate style of ornamentation. It was amongst the ornamental designs of those races that Professor Haddon commenced in earnest a study of the life-histories of the designs found in native art, and the results of that study he gave us in the form of a valuable memoir in the Cunningham Series of the Royal Irish Academy. In this book the author has gone much further afield, and has written what is practically an introduction to the study of decorative designs all the world over. His British New Guinea researches form the nucleus of the book and give him the clue to many of his deductions. The essence of his method may be said to lie in studying each design separately, tracing its origin to some prototype, observing the meaning attached to it, working out its life-history, noting the transmutations it undergoes, and collecting the intermediate forms of the devices until he gets a series that connects the final meaningless conventionality with the real and living form from which it has evolved. The material from which designs are drawn gives him a basis for their classification. All designs, he finds, may be classified either under *zoomorphs*, those derived from animal forms, or *anthropomorphs*, those obtained from human forms, or *phyllomorphs*, those originating from plant forms, or *physicomorphs*, devices drawn from the material universe, or *skeuomorphs*, decorations obtained from copying forms of handiwork already in existence. From these sources, or a combination of them, often aided by a fantastic imagination, saving perhaps some plain and geometrical designs, savage and civilised people alike have drawn all their designs.

Nearly all the life-histories of designs given by the author are convincing and of extreme interest. The crocodile device, the frigate-bird, the face, the scroll, the lotus flower, the fylfot, and many other designs, are well worked out and amply illustrated. It is often extremely difficult, sometimes impossible, to trace designs to their birth-places, although there can be no doubt that a knowledge of the fauna, the flora, and ethnology of a district is of the greatest assistance in the search. The author agrees with others in thinking that many of the simpler designs may have arisen independently in different quarters of the globe, such, for instance, as the scroll design.

A student of the more æsthetic side of art, however, might have some objections to offer to certain parts of the book. He would object, probably, to the classification Professor Haddon gives of the reasons for which objects are decorated. The reasons given are (1) for *art*, (2) for *information*, (3) for *wealth*, and (4) for *religion*. Art here stands for any combination of line, form, and colour, giving a pleasurable sensation. It is this pleasurable sensation that calls all decorations into existence, and this is the only reason for the decoration of objects. Information, wealth, and religion do preserve and keep designs and devices in existence, but when designs become utilised with these significations they then cease in reality to be decorations. Such a student might also carp somewhat at the title of the book; *art* and *evolution* have become words of so loose connotation that they are almost as conventional and meaningless as some of the designs dealt with by the author. The student of the æsthetic side of art, also, would be inclined to think that Professor Haddon claims rather too much for Biology and its methods. It is quite true that designs are the outcome of the living protoplasm of the human brain, but if for this reason biologists are to claim art as a department of their subject, then must mathematics, physics, history, and every science and art that the human mind deals with, fall to their share. Nor is there anything peculiar in the methods of the biologist; he uses his eyes, makes records, collects facts, and draws deductions just as every other scientist does. But this book may be taken as a proof that there is no reason why a biologist may not be a successful student of art and write a book upon the subject charmingly free from all self-seeking, and making full and open acknowledgment of the debt he owes to the observations and conclusions of men that have already worked at this subject.

# APPENDIX II.

## CHEMICAL LITERATURE FOR MAY, 1896.

### Vol. i. No. 5. *American Journal of Science.* (May, 1896.)

*Trowbridge, J.*, Carbon and Oxygen in the Sun (pp. 329-333). *Washingon, H. S.*, Ischian Trachytes (pp. 375-386). *Lea, M.C.*, Numerical Relations existing between the Atomic Weights of the Elements (pp. 386-389). *Palache, C.*, Crocoite from Tasmania (pp. 389-391).

### Vol. xviii. No. 5. *Journal of the American Chemical Society.* (May, 1896.)

*De Chalmot, G.*, Hydrofluoric Acid (pp. 415-425). *Coates, C. E.*, and *Dodson, W. R.*, Nitrogen Assimilation in the Cotton Plant (pp. 425-428). *Wiley, H. W.*, and *Ewell, E. E.*, Determination of Lactose in Milks by Double Dilution and Polarisation (pp. 428-434). *Venable, F. P.*, and *Clarke, T.*, A Study of the Zirconates (pp. 434-445). *Winton A. L.*, A Modified Ammonium Mobybdate Solution (pp. 445-446). *Gladding, T. S.*, On the Estimation of Sulphur in Pyrites (pp. 446-449). *De Schweinitz, E. A.*, and *Dorsett, M.*, Further Notes upon the Fats contained in the Tuberculosis Bacilli (pp. 449-451). *McIllhiney, P. C.*, The Cassel-Hinman Gold and Bromine Process (pp. 451-457). *Lord, N. W.*, A Simple Method for Determining the Neutrality of the Ammonium Citrate Solution used in the Analysis of Fertilisers (pp. 457-458). *Low, A. H.*, The Copper Assay by the Iodide Method (pp. 458-462). *Shorey, E. C.*, On Two Sources of Error in Sugar House Analyses (pp. 462-466).

### Vol. xviii. No. 4. *American Chemical Journal.* (April, 1896.)

*Fay, H.*, The Action of Light on some Organic Acids in the Presence of Uranium Salts (pp. 269-290). *Herty, C. H.*, A Review of Some Recent Work on Double Halides (pp. 290-294). *Campbell, E. D.*, and *Hart, E. B.*, On the Quantitative Determination of Hydrogen by Means of Palladous Chloride (pp. 294-298). *Jackson, C. L.*, and *Calvert, C.*, On the Behaviour of Certain Derivatives of Benzol Containing Halogens (pp. 298-312). *Orndorff, W. R.*, and *Howells, V. A.*, The Cis and Trans Modification of Benzene Hexabromide (pp. 312-319). *De Chalmot, G.*, Silicide of Calcium (pp. 319-321). *Jones, H. C.*, and *Allen, C. R.*, The Conductivity of Yttrium Sulphate (pp. 321-323). *Walker, M. S.*, The Practical Use in the Chemical Laboratory of the Electric Arc obtained from the Low Potential Alternating Current (pp. 323-328). *Keiser, E. H.*, The Preparation of Allylene, the Action of Magnesium upon Organic Compounds (pp. 328-332). *Dunlap, F. L.*, The Action of Urea and Sulphocarbanilide on Certain Acid Anhydrides (pp. 332-341).

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*Remsen, I.*, and *Muckenfuss, A. M.*, Transformations of Parasulphaminebenzoic Acid under the Influence of Heat (pp. 349-365). *Kortright, F. L.*, The Heat of Electrolytic Dissociation of Some Acids (pp. 365-372). *Lachman, A.*, On the Existence of Pentaethyl Nitrogen (pp. 372-375). *Jones, H. C.*, and *Allen, C. R.*, The Conductivity of Solutions of Acetylene in Water (pp. 375-377). *Jones, H. C.*, and *Allen, C. R.*, The Use of Phenolphthalein in Illustrating the Dissociating Action of Water (pp. 377-381). *Wheeler, H. L.*, and *Boltwood, B. B.*, The Action of Acid Chlorides on the Silver Salts of the Anilides (pp. 381-390). *Howe, W. T. H.*, On the Existence of Two Orthophthalic Acids (pp. 390-401). *Morse, H. N.; Hopkins, A. J.;* and *Walker, M. S.*, The Reduction of Permanganic Acid by Manganese Superoxide (pp. 401-420).

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*Pearmain, T. H.*, and *Moor, C. G.*, The Bacteriological Examination of Water for the Typhoid Bacillus (pp. 117-122). *Sykes, W. J.*, and *Mitchell, C. A.*, The Estimation of the Diastatic Power of Malt, etc. (pp, 122-128). *Shepherd, H. H. B.*, Official Methods for the Analysis of Fertlizers, issued by the German Manure Manufacturers Association (pp. 128-133). *Reed, L.*, Tropaeölins in Milk, etc. (p. 140).

Vol. xv. No. 4. *Journal of the Society of Chemical Industry.* (30th April, 1896.)

*Sindall, R. W.*, Moisture in Wood Pulp (pp. 239-245). *Smith, W.*, A Study of Comparative Affinities in the case of Certain Salts of Ammonium and Wool. II. (pp. 245-247). *Herman, D.*, On Poisoning by Gas: its Prevention and Cure (pp. 247-248). *Kohn, C. A.*, A Modified Form of Schrötter's Apparatus for the Determination of Carbonic Anhydride (p. 248). *Beveridge, J.*, The Wood Cellulose Industry of Scandinavia (pp. 249-251). *Beveridge, J.*, On a Method of Calculating the Amount of Steam required to Dry a Ton of Paper or Pulp (pp. 251-252). *Hart, P.*, Description of a Simple Feed-Water Heater (pp. 252-253). *Flintoff, R. J.*, The Functions of Albumin as a Fixing Agent for Pigments on Cotton (pp. 253-254). *Barnes, J.*, On the Preparation of Water Free from Ammonia (pp. 254-255). *Clark. J.*, Estimation of Antimony in Ores and Metals (pp. 255-257). *Dewey, F. P.*, The Sulphuric Acid Process of Refining Lixiviation Sulphides (pp. 257-260).

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*Shields, J.*, A Mechanical Device for Performing the Temperature Corrections of Barometers (pp. 406-414). *Wood, R. W.*, On the Absorption Spectrum of Solutions of Iodine and Bromine above the Critical Temperature (pp. 423-431). *Trowbridge, J.*, Carbon and Oxygen in the Sun (pp. 450-454).

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*Wildermann, M.*, A new Method of determining Freezing Points (pp. 251-254). *Collie, J. N.* and *Ramsay, W.*, On the Behaviour of Argon and Helium when submitted to the Electric Discharge (pp. 257-270). *Gamgee, A.*, On the Absorption of the extreme Violet and ultra-Violet Rays of the Solar Spectrum by Haemoglobin, its Compounds, and certain of its Derivatives (pp. 276-279).

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*Parenty, H.*, Sur le débit des gaz parfaits et de la vapeur d'eau sous pression à travers les orifices (pp. 5-79). *Varet, R.*, Recherches sur les sels de mercure (pp. 79-141). *Moissan, H.*, Étude sur l'argon (pp. 141-144).

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*Thomas, V.*, Action du peroxyde d'azote et de l'air sur le chlorure de bismuth (pp. 469-472). *Granger, A.*, Sur un sulfophosphure d'étain (pp. 472-474). *Guye, A.*, et *Jordan, C.*, Recherches expérimentales sur les butanol- 2- oïques (α-oxybutyrique) actifs (pp. 474-498). *Marie, E.*, Éthers glycériques des acides cérotique et mélissique (pp. 498-503). *Marie, T.*, Chlorures d'acides, amides, nitriles des acides cérotique et mélissique (pp. 503-508). *Marie, T.*, Sur l'oxydation des acides des graisses (pp. 508-510). *Marie, T.*, Sur les propriétés des acides cérotique et mélissique existant à l'état libre dans la cire d'abeilles (pp. 510-514). *Vaillant, V.*, Sur quelques dérivés métalliques de la dithioacétylacétone (pp. 514-519). *Marcourt, E.*, Étude chimique d'un nouveau composé d'antipyrine at d'aldéhyde formique (pp. 520-529). *Reychler, A.*, Étude sur un dibromure de camphène (pp. 529-530). *Scheurer-Kestner*, Sur la détermination de l'acidité des produits pyroligneux (pp. 530-536).

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*Guye, P. A.*, et *Goudet, C.*, Superposition optique de six carbones asymétriques dans une même molécule active (pp. 932-935). *Didier, G.*, Sur un azotate basique de magnésie (pp. 935-936). *Granger, A.*, Sur le sesquiphosphure de fer cristallisé (pp. 936-937). *Gassmann, C.*, Étude sur le péridinitronaphtalène (pp. 937-940). *Causse, H.*, Sur le tartrate du phényl-hydrazine et ses dérivés (pp. 940-943). *Guinchant*, Chaleur de combustion des dérivés cyanés (pp. 943-946). *Sorel, E.*, Sur la distillation des premiers acides de la série grasse (pp. 946-948). *Friedel, G.*, Sur les zéolithes et la substitution de diverses substances à l'eau qu'elles continnent (pp. 948-951). *Henry, C.*, Sur la détermination par une méthode photométrique nouvelle, des lois de la sensibilité lumineuses aux noirs et aux gris (pp. 951-954). *Gérardin, A.*, et *Nicloux, M.*, Mesure des odeurs de l'air (pp. 954-955).

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*Bertrand, J.*, Sur la théorie des gaz (pp. 963-967). *Cazeneuve, P.*, Sur un nouveau mode de préparation synthétique de l'urée et des urées composées symétriques (pp. 999-1000). *Arnaud, A.*, Transformation de l'acide taririque et de l'acide stéaroléique en acide stéarique (pp. 1000-1002). *Bourquelot, E.*, Sur la présence, dans le *Monotropa Hypopithys*, d'un glucoside de l'éther méthylsalicylique et sur le ferment hydrolysant de ce glucoside (pp. 1002-1004). *Balland*, Sur le maïs (pp. 1004-1006).

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l'agrostemme dans la farine (pp. 143-149). *Reformatsky, S.*, Sur la préparation de l'acide αα-diméthylglutarique (pp. 149-159). *Dain, G.*, Action du zinc et de l'éther bromoisobutyrique sur l'aldehyde benzoique (pp. 159-175). *Jacovkin, A.*, Sur la répartition d'un composé entré deux dissolvants (pp. 175-186). *Soboleff, M.*, Sur les proprietés de l'acide phospho- 12- wolframique (pp. 186-207). *Beketoff, N.*, Sur le changement du volume ayant lieu lors de la formation de l'iodure d'argent en partant des éléments; *Kournakoff, N.*, Sur la structure des formes les plus complexes des composés chimiques; *Gvosdareff, N.*, Sur les combinaisons ethylènediamidiques de palladium; *Zelinsky, N.*, et *Tchernosvitoff, N.*, Sur les acides diméthyltricarballyliques; *Rabinovitsch, I.*, Analyse de l'eau du lac salé de Golopristan; *Tschernik, G.*, Sur un mineral ceritique du district de Batoum; *Dementieff, K.*, Dosage volumetrique du zinc (pp. 207-222).

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*Peinemann, K.*, Beiträge zur pharmacognostichen und chemischen Kenntnis der Cubeben und der als Verfälschung derselben beobachteten Piperaceenfrüchte (pp. 241-272). *Lewin, L.*, Uber eine forensische Strychninuntersuchung (pp. 272-273). *Kiliani, H.*, Ueber den Nachweis der Digitalis-glycoside und ihrer Spaltungsprodukte durch eisenhaltige Schwefelsäure (pp. 273-278). *Mjöen, J. A.*, Ueber das fette Oel des *Secale cornutum* (pp. 278-283). *Mjöen, J. A.*, Ueber das fette Oel der Samen von *Strophantus hispidus* (pp. 283-286). *Mjöen, J. A.*, Ueber das fette Oele der Samen von *Hyoscyamus niger* (pp. 286-289). *Balzer, A.*, Ueber das Sandaracharz (pp. 289-317). *Otto, R.*, Das Verhalten des Papaverins und Narkotins bei dem Stas - Otto'schen Verfahren der Ausmittelung der Alkaloide (pp. 317-320).

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*Senkowski, M.*, Zur Kenntniss der Constitution der Cholsäure (pp. 1-4). *Georgievics,* G. *v.* Zu Kenntniss der gefärbten Rosanilinbasen (pp. 4-13). *Meyerhoffer, W.*, Ueber reciproke Salzpaare I. (pp. 13-29). *Mauthner, J.*, und *Suida, W.*, Beiträge zur Kenntniss des Cholesterins III. (pp. 29-50). *Eder, J. M.*, und *Valenta, E.*, Ueber drei verschiedene Spectren des Argons (pp. 50-57). *Jeiteles, B.*, Ueber die Destillation von *o*- Kresol mit Bleioxyd (pp. 57-65). *Jeiteles, B.*, Notiz, ueber das Verhalten von phenylsalicylsaurem Calcium bei der trochenen Destillation (pp. 65-68). *Lieben, A.*, Ueber die durch Einwirkung von alkoholischen Kali auf Aldehyde entstehenden zweiwerthigen Alkohole (pp. 68-76). *Just, A.*, Einwirkung von alkoholischen Kali auf ein Gemenge von Formaldehyd und Isobutyraldehyd (pp. 76-85). *Franke, A.*, Ueber das aus dem Isobutyraldehyd entstehende Glykol und dessen Derivate (pp. 85-102). *Cohn, P.*, Ueber o- Benzoylphenol (pp. 102-109).

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*Behrens, H.*, Zur mikrochemischen Unterscheidung von Cinchonidin und Homocinchonidin (pp. 133-143). *Ruoss*, Eine allgemeine volumetrische Bestimmung der durch fixe äbzende oder kohlensaure Alkalien fällbaren Metalle (pp. 143-159). *Stutzer, A.*, und *Maul, R.*, Untersuchung von Feinsprit auf dessen Gehalt an Fuselöl (pp 159-163). *Vaubel, W.*, Zur Gehaltsbestimmung von Benzidin und Tolidin (pp. 163-164). *Vaubel, W.*, Ueber das Verhalten der Naphtole und Naphtylamine gegen nascirendes Brom (pp. 164-166). *Strohl, A.*, Jodzahl und Brechungsindex der Cacaobutter (pp. 166-169). *Greiner* und *Freidrichs*, Aräometer- Pipette (pp. 169-170). *Bornträger, H.*, Ein Verfahren zur Auflösung des geglühten Eisenoxydes und anderer Metalloxyde (p. 170). *Fresenius, R.*, und *Hintz, R.*, Ueber eigenthümliche Löslichkeitsverhaltnisse des schwefelsauren Baryts (pp. 170-184).

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*Thomsen, J.*, Experimentelle Untersuchung ueber die Dichte des Wasserstoffes und des Sauerstoffe (pp. 1-16). *Sobolew, M.*, Ueber einige physikalische Eigenschaften der Phosphor 1.2. Wolframsäure (pp. 16-39). *Goldhammer, D. A.*, Bemerkungen ueber die analytische Darstellung des periodischen Systems der Elemente (pp. 39-46). *Werner, A.*, Ueber eine eigentümliche Klasse von Platinverbindungen und die sogenannten isomeren Platosoxalsäuren (pp. 46-55). *Hofmann, K. A.*, Eine neue Persulfomolybdänsäure (pp. 55-63).

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*Luther, L.*, Elektromotorische Kraft und Verteilungsgleichgewicht (pp. 529-572). *Ihle, R.*, Ueber die Bildung von Ammoniak bei der Elektrolyse der Saltpetersäure (pp. 572-577). *Ihle, R.*, Ueber die katalytische Wirkung der salpetrigen Säure und das Potential der Salpetersäure (pp. 577-592). *Tafel, J.*. Ueber die sogenannte "indirekte Esterbildung" (pp. 592-599). *Noyes, A. A.*, Die katalytische Wirkung der Wasserstoffionen auf polymolekular Reaktionen (pp. 599-607). *Wald, F.*, Die Genesis der stöchiometrischen Grundgesetze. II. (pp. 607-625). *Kenrich, F. B.*, Die Potentialsprünge zwischen Gasen und Flüssigkeiten (pp. 625-657). *Friedländer, S.*, Ueber Argon (pp. 657-668). *Wagner, M.*, Die Zersetzungsgeschwindigkeiten der Schwefel-stickstoffsäuren (pp. 668-689). *Wood, R. W.*, Ueber die Absorptionsspektren der Lösungen von Jod und Brom ueber der Kritrischen Temperatur (pp. 689-696). *Tanatar, S.*, Die Lösungs- und Neutralisationswärme des Nitroharnstoffes und seines Kaliumsalzer (pp. 696-699). *Schall, C.*, Nachtrag zur letzten Abhandlung (pp. 699-709).

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*Moro, P.*, Ricerche sull'acido naftalindicarbonico 1-5 e suoi derivati (pp. 89-116). *Zoppellari, I.*, Sopra alcuni fenomeni osservati nel congelamento di soluzioni diluite (pp. 116-119). *Carrara, G.*, Per la teoria della dissociazione elettrolitica in solventi diversi dall'aqu. I. Alcool metitico (pp. 119-197).

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# APPENDIX I.

## NOTICES OF BOOKS.

*Grundriss der Krystallographie für Studirende und zum Selbstunterricht.* By Gottlob Linck. Jena, 1896. Pp. vi. and 252.

The great and ever-increasing importance of the position held by crystallography as an auxiliary to the kindred sciences of physics and chemistry is gradually receiving recognition. Indications are not wanting that in the future many of the problems of molecular physics relating to solid bodies will be found easily susceptible to attack by crystallographic methods, so that crystallography will at length take its legitimate position as a branch of physical chemistry. With these prospects, we gladly welcome any book in which crystallography is rationally discussed as a living science, which embraces a remarkably fertile and hitherto comparatively unexploited field.

In the preface to the present volume, the author states that his work is designed for the perusal of young students; and, in spite of what appear to us serious defects, the book is of an extremely readable character, and its language so clear and simple, that the author's design is, on the whole, well carried out. We find, however, no mention of the stereographic projection throughout the work, and an English reader can scarcely conceive of a student attaining to an appreciative grasp of elementary crystallography, without early acquiring an easy facility in the use of that simple and invaluable aid. Surely, too, the time has come when Naumann's cumbrous method of describing forms might well be consigned to the crystallographic historian, and be entirely omitted from educational works. Here, however, the author assigns to it equal prominence with the more significant and elegant Millerian method.

That portion of the work which deals with the geometrical properties of crystals is concise and well arranged. The method of treating the hexagonal system is, however, unsatisfactory, the reader being left with very vague notions respecting the precise meaning and character of the axial system employed. The treatment of crystallographic optics is clear, and, for an elementary manual, leaves little to be desired. The chapter dealing with polymorphism, isomorphism, morphotropy, etc., is a very praiseworthy innovation in a work of this kind; it treats briefly and concisely of the relations existing between chemical composition and crystalline form.

The book is well printed and amply illustrated with diagrams of a distinctly superior character to those which we are accustomed to see copied from one work into another, *ad nauseum;* a coloured diagram of interference figures ends the book.

The following list of errata may be of service :—

| | | | | | | |
|---|---|---|---|---|---|---|
| P. | 6, | line | 12 | from below, | *read* | Krystallsystem. |
| ,, | 36, | ,, | 14 | ,, above, | ,, | Triakisoctaëder. |
| ,, | 43, | ,, | 17 | ,, below, | ,, | einander. |
| ,, | 64, | ,, | 11 | ,, above, | ,, | Nebensym. |
| ,, | 188, | ,, | 9 | ,, below, | ,, | chroïsmus. |
| ,, | 190, | ,, | 18 | ,, ,, | ,, | $Mg\ Al_2\ O_4$. |
| ,, | 192, | ,, | 21 | ,, above, | ,, | nach. |
| ,, | ,, | ,, | 35 | ,, ,, | ,, | convergent. |
| ,, | 193, | ,, | 16 | ,, ,, | ,, | gewöhnlichem. |
| ,, | 195, | ,, | 22 | ,, below, | ,, | differenz. |
| ,, | 198, | ,, | 33 | ,, above, | ,, | 441. |
| ,, | 203, | ,, | 14 | ,, ,, | ,, | isotropen. |
| ,, | 240, | ,, | 13 | ,, below, | ,, | Auslöschungs. |

# APPENDIX II.

## CHEMICAL LITERATURE FOR JUNE, 1896.

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